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97 8 x 6 3/8

THE AMERICAN NATURALIST

A MONTHLY JOURNAL

DEVOTED TO THE NATURAL SCIENCES IN THEIR WIDEST SENSE.

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Vol. XXX. JULY, 1896. No. 355

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PHILADELPHIA, U. S. A.

THE EDWARDS & DOCKER CO.,

518 AND 520 MINOR STREET.

Entered at the Philadelphia Post Office as second-class matter.

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v. 30
July-Dec.
1896

THE

AMERICAN NATURALIST

VOL. XXX. July, 1896. 355

THE CLASSIFICATION OF DIATOMS (BACILLARIACEÆ).[1]

BY CLARENCE J. ELMORE.

There have been many systems of classification employed for the *Bacillariaceæ*, but very few of these have any valid claim, to be regarded as natural systems. They may be divided into three classes; (1) those based on the structure of the valves, of which Kuetzing's, Prof. H. L. Smith's, and that employed by Kirchner are examples; (2) those based on the form of the frond, the connecting membrane, and the gelatinous envelope, represented by Rev. Wm. Smith's; and (3) those based on the structure of the endochrome and the manner of forming auxospores, represented by that of Paul Petit. The following is a brief outline of the systems mentioned.

Kirchner divided the *Bacillariaceæ* into two groups[2] those whose markings are bilateral, that is, arranged on two sides of a longitudinal line or raphe, and[3] those with radial markings.

[1] Read before the Botanical Seminar of the University of Nebraska, March 21. 1896.

[2] Kryptogamen-Flora von Schlesien; Algen, 171 Breslau, 1878.

[3] Species Algarum, 1. Leipsic, 1849.

Those with bilateral markings he divided into two subdivisions the first comprising those with a central nodule, and the second those with none.

Kuetzing divided the *Bacillariaceæ* into three tribes; I, *Striatæ*, that is those with transverse striations; II, *Vittatæ*, that is those with longitudinal stripes; and III, *Areolatæ*, that is those whose surfaces are divided into angular areolæ. The first-two tribes, *Striatæ* and *Vittatæ* he divided into two orders each, viz.; I, *Astomaticæ* and II, *Stomaticæ*. The *Astomaticæ* included those with no central nodule, or as he understood it, with no central opening, while the *Stomaticæ* included those with a central nodule. If the central nodule were really a stoma or aperture as Kuetzing considered it, this grouping might have been a natural one; for this difference in structure might have connoted important physiological differences, but it is generally conceded that the nodules are merely markings on the valves, and it is likely that they indicate nothing as to the physiology of the plant. So no higher groups than genera, or possibly species, can be based on this character. His third tribe, *Areolatæ*, he also divided into two orders; I, *Disciformæ* that is, those of a circular or angular form, and, II, *Appendiculatæ*, or forms with appendages, as *Biddulphia*.

The classification of Prof. H. L. Smith[4] is one that has had considerable following. Bessey's Botany[5] was the first American textbook to adopt and give an outline of the system. It was adopted by Van Heurck[6], Wolle[7] and De Toni[8]. To say the least, it is a good practical system of classification, and probably this is the most that can be said for it, though in some points it seems to approach a natural system. Smith divides the Diatoms into three tribes, the *Raphideæ*, *Pseudoraphideæ*, and *Cryptoraphideæ*. The *Raphideæ* are all supposed to possess a raphe. The *Pseudoraphideæ* are usually elongated, have no raphe, but in its place there is a blank space resembling a

[4] Conspectus of the Families and Genera of the Diatomaceæ in *The Lens*, I: 1 1872 and II: 65, 1873.

[5] Botany for High Schools and Colleges, Henry Holt and Co., New York, 1880,

[6] Synopsis des Diatomées de Belgique, 1885.

[7] Diatomaceæ of North America, 1890.

[8] Sylloge Algarum, 1891.

raphe. The *Cryptoraphideæ* are usually circular or angular and have nothing resembling a raphe. Upon the supposition that the raphe is an essential organ, and that it is present in one tribe, replaced by another structure in the second, and "hidden" in the third, this might be a natural classification. But if the raphe is known to exist only in the first tribe and its existence in the others is wholly theoretical, it will hardly serve as a character on which to base a classification. It is true that the genera brought together by this system appear to bear more or less relation to each other, but if we knew as little about Phanerogams as we do about Diatoms, we should think that a division of them into *Arboræ*, *Frutices*, and *Herbæ* placed related genera together, for it would be easy to see that *Salix* and *Populus* are related, and also that *Solanum* and *Physalis* are more or less closely allied. I venture to regard the *Raphideæ*, *Pseudoraphideæ*, and *Cryptoraphideæ* as having no greater naturalness than the divisions *Arboræ*, *Frutices*, and *Herbæ;* and it is to be hoped that they will soon be consigned to the same botanical limbo in which the latter have long since found obscurity.

It is true, however, that in the *Raphideæ*, there seems to be a trace of naturalness in the system. The author begins with the bilaterally symmetrical forms, that is those in which the raphe is a median line, as for example, *Navicula*. Those with the raphe at one side of the center, as in *Cymbella*, he considers a modification of the first type by a curving of the frustule and thus bringing the raphe nearer the concave side. And in the third division the raphe has approached so near to the concave margin that it fuses with it, as in *Amphora*. If this is to be considered simply as a modification of a typical form, it means little. But if this modification shows the course of development from the *Navicula* form to the *Amphora* form, it means a great deal. In *Navicula* and *Cymbella* two auxospores are formed from two mother cells without conjugation, and in *Amphora* two auxospores are formed from two mother cells by conjugation. It is probable that the method of reproduction found in the derived form is a development from that found in the primitive form. If then the *Amphora* form has developed from the *Navicula* form, there is reason to believe that the for-

mation of auxospores without conjugation is the primitive method, although Murray[9] holds that the formation of auxospores by conjugation is probably the original method, and that their formation without conjugation is the derived method.

Wm. Smith[10] divided the Diatoms into two tribes in the first of which the frustules are free, and in the second imbedded in a gelatinous envelope. Under the first tribe he makes five subtribes, depending upon the form of the connecting membrane and the relation of the frustules to each other. The second tribe he divided into four subtribes based on the form of the fronds. This arrangement seems not only extremely artificial but also very impractical. Nothing about Diatoms is more variable than the form of the fronds; and where it is at all constant, such a system places closely related genera far apart; for example, *Cymbella* and *Encyonema*, *Nitzschia* and *Homœocladia* are placed in separate tribes, while in structure they are very similar, the main difference being that in *Encyonema* and *Homœocladia* the frustules are arranged in rows, while in *Cymbella* they are free or stipitate and in *Nitzschia* they are free. This method of classifying Diatoms may be likened to a separation of Grasses into those forming a dense sod and those not forming a sod; or of Dicotyledons into those exuding a resinous fluid and those that do not. Wm. Smith places *Gomphonema* in his first tribe, that is, the one having no gelatinous envelope; but some species of *Gomphonema* are stipitate while others are enclosed in an amorphous mass of jelly. The latter species would have to be placed in his second tribe, thus dividing the genus. It would lead to even greater difficulty than this, for the same species is sometimes stipitate and sometimes imbedded in a gelatinous envelope.

Of all existing systems that of Paul Petit[11] seems to approach

[9] An Introduction to the Study of Seaweeds, p. 195, 1895.

[10] For a synopsis of Smith's classification see Pritchard's History of the Infusoria, 101, fourth edition, 1861.

[11] Liste des Diatomées et des Desmidées observées dans les Environs de Paris precedée d'un essai de classification des Diatomées. Bull. Soc. Bot. France, tom. XXIII–XXIV, Paris, 1877.

An Essay on the Classification of the Diatomaceæ translated by F. Kitton, Monthly Microscopical Journal and Transactions of the Royal Microscopical Society, XVIII, 1877, pp. 10, 65.

Pfitzer, Die Bacillariaceen, in Schenk's Handbuch der Botanik, Breslau, 1882.

most nearly to a natural one because it is based on characters having physiological significance. It is based primarily on the structure of the endochrome, and secondarily on the method of forming auxospores and the general shape of the frustules. Van Heurck does not employ this system in his *Synopsis* because of the large number of fossil specimens and those from deep-sea soundings to which it could not be applied. But this is not a valid objection, for all the genera are represented by modern species, and these are sufficient for a basis of classifications, and since the specific characters are based mainly on the structure of the valves, there will be no trouble with the fossil forms. The following synopsis of Petit's system includes the higher divisions only.

I. *Bacillariaceæ coccochromaticæ.*

With numerous endochrome granules.

A. Frustules concentrically constructed. One mother cell forming asexually a single auxospore. *Melosireæ*, etc.

B. Frustules bilateral, one or two mother cells forming two auxospores, as far as known asexually. *Fragilarieæ*, etc.

II. *Bacillariaceæ placochromaticæ.*

With one or two large endochrome plates.

A. One endochrome plate lying against the convex valve; one mother cell forming one auxospore asexually. *Cocconeideæ.*

B. A single endochrome plate extending diagonally across the cell cavity, or lying next the girdle. Two auxospores formed from two mother cells, with or without conjugation. *Nitzschieæ. Amphoreæ, Cymbelleæ*, etc.

C. Two endochrome plates lying next the two valves. Two mother cells forming two auxospores by conjugation. *Eunotieæ, Synedrieæ, Surirayeæ.*

D. Two endochrome plates lying next the two girdle bands; two mother cells forming two auxospores without conjugation. *Amphipleureæ, Naviculeæ*, etc.

Although Petit's system is by no means perfect, it is at least a step in the right direction. He bases it upon characters that have some physiological significance, while the other systems are wholly or in greater part based on merely accidental characters. A clue to the genetic relationships of Diatoms, as of other plants, will be most certainly found in their method of reproduction. The shape of the frustules, or their markings, will serve for specific, or in some cases for generic characters, but they have no significance that will warrant their use in the erection of higher groups. Absolute shape and size will not serve as definite characters, for a single species between one auxospore stage and the next varies greatly in both these respects. Owing to the peculiar mode of cell division in which each new valve is formed inside the old one, each new frustule is smaller than the parent, hence the size gradually decreases until an auxospore is formed. Schumann[12], out of 470 species found ten in which the length of the largest was five times that of the smallest; twenty-nine in which the largest were from three to four times as long as the smallest, and the rest showing less variation. The variation in form is even as great as the variation in size. This is probably due to the difference in the thickness of the girdle, *i. e.* the part of the valves that overlaps, in different parts of the frustule. *Navicula iridis* Ehr. is a good example of a variable species. Its different forms have been described as species by most writers. In the typical form the valves are elliptical with gracefully curved margins. The first variation from this type has apices cuneate, and a still further deviation shows them acuminate-cuneate; and from this it varies to rostrate or capitate; and a diminution in size goes step by step with this change in form. These forms are represented by *Navicula iridis* Ehr., *N. amphigomphus* Ehr., *N. affinis* Ehr., *N. amphirhynchus* Ehr., and *N. producta* W. Sm. If the overlapping portions of the valves are slightly thicker near the ends than elsewhere, this variation would be the necessary result, for each new valve formed inside an old one would be slightly constricted opposite this thickened place, at first changing the rounded ends to cuneate, and as the narrowing pro-

[12] Pfitzer, l. c., p. 441.

ceeded still further, the cuneate form would become rostrate and a still further narrowing would give a capitate form. So form and size, although they have a certain significance, are not to be considered infallible characters.

The geological records throw no light upon the relationship of the *Bacillariaceæ*, for when this family first appeared, we find the same genera, and largely the same species as in our modern ones. This is probably due to the fact that their ancestors lacked the siliceous covering, and hence were not preserved. Diatoms evolved the same as all other plants until they developed their shells, but these put a stop to their further evolution, at least they show no trace of evolution since their first appearance. So the question arises whether the Diatoms represent the ends of several closely related genetic lines the further development of which was stopped by their siliceous shells, or whether we may trace the development of one form from another. The former supposition is the more probable, for the form of the earliest fossil specimens is identical with that of modern specimens of the same species; and the same genera are found among fossil as among modern Diatoms. If one genus of Diatoms developed from another, we ought to find the more primitive forms in the earlier strata, for there is little chance that their remains would not be preserved had they existed. But instead of this, Diatoms of all forms appear almost simultaneously. We may conclude then that the *Bacillariaceæ* represent the silicified ends of several closely allied genetic lines and that they have not changed in form since they acquired their siliceous covering. The structure of the valves it follows will tell us practically nothing of their relationship.

There are five methods by which auxospores are formed[13]. In the first the protoplasm of one frustule simply escapes from the valves, grows to a certain size, and then invests itself with new valves. In the second, two auxospores, instead of one, are formed in the same way by the dividing of the protoplasm of a single plant. In the third, the protoplasm of two Diatoms unites to form an auxospore. In the fourth, the protoplasm of

[13] Murray, l. c.

two Diatoms emerges from the valves, and placed by side, but without conjugation, forms each an auxospore. In the fifth, two Diatoms divide transversely and the two halves of each conjugate, each half with the corresponding half of the other and thus form two auxospores. Before any truly natural classification can be made the significance of these various modes of producing auxospores must be understood. Whether the sexual or the asexual method is the primitive one must be known, or whether the different methods are so many expedients to overcome the difficulties imposed upon these plants by their siliceous shells. At present our knowledge of the structure and physiology of Diatoms is not sufficient to enable us to construct a perfectly natural system of classification, and until something better is proposed, Petit's may well be adopted, for although it is not wholly natural, it is more so than any which has preceded it.

A NEW FACTOR IN EVOLUTION.

By J. Mark Baldwin.

(*Continued from page 451*).

III.

Social Heredity.—There follows also another resource in the matter of development. In all the higher reaches of development we find certain co-operative or "social" processes which directly supplement or add to the individual's private adaptations. In the lower forms it is called gregariousnes, in man sociality, and in the lowest creatures (except plants) there are suggestions of a sort of imitative and responsive action between creatures of the same species and in the same habitat. In all these cases it is evident that other living creatures constitute part of the environment of each, and many neuro-genetic and psycho-genetic accommodations have reference to or involve these other creatures. It is here that the principle of imitation gets tremendous significance; intelligence and vol-

ition, also, later on; and in human affairs it becomes social co-operation. Now it is evident that when young creatures have these imitative, intelligent, or quasi-social tendencies to any extent, they are able to pick up *for themselves*, by imitation, instruction, experience generally, the functions which their parents and other creatures perform in their presence. This then is a form of ontogenetic adaptation; it keeps these creatures alive, and so produces determinate variations in the way explained above. It is, therefore, a special, and from its wide range, an extremely important instance of the general principle of Organic Selection.

But it has a farther value. *It keeps alive a series of functions which either are not yet, or never do become, congenital at all.* It is a means of extra-organic transmission from generation to generation. It is really a form of heredity because (1) *it is a handing down of physical functions*; while it is not physical heredity. It is entitled to be called heredity for the further reason (2) that *it directly influences physical heredity in the way mentioned*, i. e., it keeps alive variations, thus sets the direction of ontogenetic adaptation, thereby influences the direction of the available congenital variations of the next generation, and so determines phylogenetic development. I have accordingly called it "Social Heredity" (ref. 2, chap. xii; ref. 3).

In "Social Heredity," therefore, we have a more or less conservative, progressive, ontogenic atmosphere of which we may make certain remarks as follows:—

(1) *It secures adaptations of individuals all through the animal world.* "Instead of limiting this influence to human life, we have to extend it to all the gregarious animals, to all the creatures that have any ability to imitate, and finally to all animals who have consciousness sufficient to enable them to make adaptations of their own; for such creatures will have children that can do the same, and it is unnecessary to say that the children must inherit what their fathers did by intelligence, when they can do the same things by intelligence" (ref. 6).

(2) *It tends to set the direction of phylogenetic progress* by Organic Selection, Sexual Selection, etc., *i. e.*, it tends not only

to give the young the adaptations which the adults already have, but also *to produce adaptations which depend upon social coöperation; thus variations in the direction of sociality are selected and made determinate.* "When we remember that the permanence of a habit learned by one individual is largely conditioned by the learning of the same habits by others (notably of the opposite sex) in the same environment, we see that an enormous premium must have been put on variations of a social kind—those which brought different individuals into some kind of joint action or coöperation. Wherever this appeared, not only would habits be maintained, but new variations, having all the force of double hereditary tendency, might also be expected" (ref. 3). Why is it, for example, that a race of Mulattoes does not arise faster, and possess our Southern States? Is it not just the social repugnance to black-white marriages? Remove or reverse *this influence of education, imitation, etc.*, and the result *on phylogeny* would show in our faces, and even appear in our fossils when they are dug up long hence by the paleontologist of the succeeding aeons!

(3) *In man it becomes the law of social evolution.* "Weismann and others have shown that the influence of animal intercourse, seen in maternal instruction, imitation, gregarious coöperation, etc., is very important. Wallace dwells upon the actual facts which illustrate the 'imitative factor,' as we may call it, in the personal development of young animals. I have recently argued that Spencer and others are in error in holding that social progress demands use-inheritance; since the socially-acquired actions of a species, notably man, are socially handed down, giving a sort of 'social heredity' which supplements natural heredity" (ref. 4). The social "sport," the genius, is very often the controlling factor in social evolution. He not only sets the direction of future progress, but he may actually lift society at a bound up to a new standard of attainment (ref. 6). "So strong does the case seem for the Social Heredity view in this matter of intellectual and moral progress that I may suggest an hypothesis which may not stand in court, but which I find interesting. May not the rise of social

life be justified from the point of view of a second utility in addition to that of its utility in the struggle for existence as ordinarily understood, the second utility, *i. e.*, of giving to each generation the attainments of the past which natural inheritance is inadequate to transmit. When social life begins, we find the beginning of the artificial selection of the unfit; and this negative principle begins to work directly in the teeth of progress, as many writers on social themes have recently made clear. This being the case, some other resource is necessary besides natural inheritance. On my hypothesis it is found in the common or social standards of attainment which the individual is fitted to grow up to and to which he is compelled to submit. This secures progress in two ways: First, by making the individual learn what the race has learned, thus preventing social retrogression, in any case; and second, by putting a direct premium on variations which are socially available" (ref. 3).

4. The two ways of securing development in determinate directions—the purely extra-organic way of Social Heredity, and the way by which Organic Selection in general (both by social and by other ontogenetic adaptations) secures the fixing of phylogenetic variations, as described above—seem to run parallel. Their conjoint influence is seen most interestingly ingly in the complex instincts (ref. 4, 5). We find in some instincts completely reflex or congenital functions which are accounted for by Organic Selection. In other instincts we find only partial coördinations ready given by heredity, and the creature actually depending upon some conscious resource (imitation, instruction, etc.) to bring the instinct into actual operation. But as we come up in the line of phylogenetic development, both processes may be present *for the same function;* the intelligence of the creature may lead him to do consciously what he also does instinctively. In these cases the additional utility gained by the double performance accounts for the duplication. It has arisen either (1) by the accumulation of congenital variations in creatures which already performed the action (by ontogenetic adaptation and handed it down socially), or (2) the reverse. In the animals, the social

transmission seems to be mainly useful as enabling a species to get instincts slowly in determinate directions, by keeping off the operation of natural selection. Social Heredity is then the lesser factor; it serves Biological Heredity. But in man, the reverse. Social transmission is the important factor, and the congenital equipment of instincts is actually broken up in order to allow the plasticity which the human being's social learning requires him to have. So in all cases both factors are present, but in a sort of inverse ratio to each other. In the words of Preyer, "the more kinds of co-ordinated movement an animal brings into the world, the fewer is he able to learn afterwards." The child is the animal which inherits the smallest number of congenital co-ordinations, but he is the one that learns the greatest number (ref. 2, p. 297).

"It is very probable, as far as the early life of the child may be taken as indicating the factors of evolution, that the main function of consciousness is to enable him to learn things which natural heredity fails to transmit; and with the child the fact that consciousness is the essential means of all his learning is correlated with the other fact that the child is the very creature for which natural heredity gives few independent functions. It is in this field only that I venture to speak with assurance; but the same point of view has been reached by Weismann and others on the purely biological side. The instinctive equipment of the lower animals is replaced by the plasticity for learning by consciousness. So it seems to me that the evidence points to some inverse ratio between the importance of consciousness as factor in development and the need of inheritance of acquired characters as factor in development" (ref. 7).

"Under this general conception we may bring the biological phenomena of infancy, with all their evolutionary significance: the great plasticity of the mammal infant as opposed to the highly developed instinctive equipment of other young; the maternal care, instruction and example during the period of dependence, and the very gradual attainment of the activities of self-maintenance in conditions in which social activities are absolutely essential. All this stock of the development theory is available to confirm this view" (Ref. 3).

But these two influences furnish a double resort against Neo-Lamarkism. And I do not see anything in the way of considering the fact of Organic Selection, from which both these resources spring, as being a sufficient supplement to the principle of natural selection. The relation which it bears to natural selection, however, is a matter of further remark below (V).

"We may say, therefore, that there are two great kinds of influence, each in a sense hereditary; there is *natural heredity* by which variations are congenitally transmitted with original endowment, and there is '*social heredity*' by which functions socially acquired (*i. e.*, imitatively, covering all the conscious acquisitions made through intercourse with other animals) are also socially transmitted. The one is phylogenetic; the other ontogenetic. But these two lines of hereditary influence are not separate nor uninfluential on each other. Congenital variations, on the one hand, are kept alive and made effective by their conscious use for intelligent and imitative adaptations in the life of the individual; and, on the other hand, intelligent and imitative adaptations become congenital by further progress and refinement of variation in the same lines of function as those which their acquisition by the individual called into play. But there is no need in either case to assume the Lamarkian factor" (ref. 4).

"The only hindrance that I see to the child's learning everything that his life in society requires would be just the thing that the advocates of Lamarkism argue for—the inheritance of acquired characters. For such inheritance would tend so to bind up the child's nervous substance in fixed forms that he would have less or possibly no unstable substance left to learn anything with. So, in fact, it is with the animals in which instinct is largely developed; they have no power to learn anything new, just because their nervous systems are not in the mobile condition represented by high consciousness. They have instinct and little else" (ref. 3).

IV.

The Process of Organic Selection.—So far we have been dealing exclusively with facts. By recognizing certain facts we have

reached a view which considers ontogenetic selection an important factor in development. Without prejudicing the statement of fact at all we may enquire into the actual working of the organism is making its organic selections or adaptations. The question is simply this: how does the organism secure, from the multitude of possible ontogenetic changes which it might and does undergo, those which are adaptive? As a matter of fact, all personal growth, all motor acquisitions made by the individual, show that it succeeds in doing this; the further question is, how? Before taking this up, I must repeat with emphasis that the position taken in the foregoing pages, which simply makes the fact of ontogenetic adaptation a factor in development, is not involved in the solution of the further question as to how the adaptations are secured. But from the answer to this latter question we may get further light of the interpretation of the facts themselves. So we come to ask how Organic Selection actually operates in the case of a particular adaptation of a particular creature (ref. 1; ref. 2, chap. vii, xiii; ref. 6, and 7).

I hold that the organism has a way of doing this which is peculiarly its own. The point is elaborated at such great length in the book referred to (ref. 2) that I need not repeat details here. The summary in this journal (ref. 6) may have been seen by its readers. There is a fact of physiology which, taken together with the facts of psychology, serves to indicate the method of the adaptations or accommodations of the individual organism. The general fact is that the organism concentrates it energies upon the locality stimulated, for the continuation of the conditions, movements, stimulations which are vitally beneficial, and for the cessation of the conditions, movements, stimulations, which are vitally depressing and harmful. In the case of beneficial conditions we find a general *increase of movement, an excess discharge of the energies of movement in the channels already open and habitual; and with this, on the psychological side, pleasurable consciousness and attention.* Attention to a member is accompanied by increased vasomotor activity, with higher muscular power, and a *general dynamogenic heightening in that member.* "The thought of a

movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement" (ref. 3). By this organic concentration and excess of movement many combinations and variations are rendered possible, from which the advantageous and adaptive movements may be selected for their utility. These then give renewed pleasure, excite pleasurable associations, and again stimulate the attention, and *by these influences the adaptive movements thus struck are selected and held as permanent acquisitions.* This form of concentration of energy upon stimulated localities, with the resulting renewal by movements of conditions that are pleasure-giving and beneficial, and the subsequent repetitions of the movements, is called the "circular reaction."[4] (ref. 1, 2). It is the selective property which Romanes pointed out as characterizing and differentiating life. It characterizes the responses of the organism, however low in the scale, to all stimulations—even those of a mechanical and chemical (physico-genic) nature. Pfeffer has shown such a determination of energy toward the parts stimulated even in plants. And in the higher animals it finds itself exactly reproduced in the nervous reaction seen in imitation and—through processes of association, substitution, etc.—in all the higher mental acts of intelligence and volition. These are developed phylogenetically as variations whose direction is constantly determined, by this form of adaptation in ontogenesis. If this be true—and the biological facts seem fully to confirm it—this is the adaptive process in all life, and this process is that with which the development of mental life has been associated.

It follows, accordingly, that the three forms of ontogenetic adaptation distinguished above—physico-genetic, neuro-genetic, psycho-genetic—all involve the sort of response on the part of the organism seen in this circular reaction with excess discharge; and we reach one general law of ontogenetic adaptation and of Organic Selection. "The accommodation of an organism to a new stimulation is secured—not by the selection of this stimulation beforehand (nor of the necessary move-

[4] With the opposite (withdrawing, depressive affects) in injurious and painful conditions.

ments)—but by the reinstatement of it by a discharge of the energies of the organism, concentrated as far as may be for the excessive stimulation of the organs (muscles, etc.) most nearly fitted by former habit to get this stimulation again (in which the "stimulation" stands for the condition favorable to adaptation). After several trials the child (for example) gets the adaptation aimed at more and more perfectly, and the accompanying excessive and useless movements fall away. This is the kind of selection that intelligence does in its acquisition of new movements" (ref. 2, p. 179; ref. 6).

Accordingly, *all ontogenetic adaptations are neurogenetic.*[5] The general law of "motor excess" is one of *overproduction;* from movements thus overproduced, adaptations survive; these adaptations set the determinate direction of ontogenesis; and by their survival the same determination of direction is set in phylogenesis also.

The following quotation from an earlier paper (ref. 7) will show some of the bearings of this position:

"That there is some general principle running through all the adaptations of movement which the individual creature makes is indicated by the very unity of the organism itself. The principle of Habit must be recognized in some general way which will allow the organism to do new things without utterly undoing what it has already acquired. This means that old habits must be substantially preserved *in the new functions;* that all new functions must be reached by gradual modifications. And we will all go further and say, I think, that the only way that these modifications can be got at all is through some sort of interaction of the organism with its environment. Now, as soon as we ask how the stimulations of the environment can produce new adaptive movements, we have the answer of Spencer and Bain—an answer directly confirmed, I think, without question, by the study both of the child and of the adult—i. e., by the selection of fit movements from excessively produced movements, that is, from *movement variations.* So granting this, we now have the further question:

[5] Barring, of course, those violent compelling physical influences under the action of which the organism is quite helpless.

How do these movement variations come to be produced *when and where they are needed?*[6] And with it, the question: How does the organism *keep those movements going* which are thus selected, and *suppress* those which are not selected?

"Now these two questions are the ones which the biologists fail to answer. But the force of the facts leads to the hypotheses of "conscious force," "self-development" of Henslow and "directive tendency" of the American school—all aspects of the new Vitalism which just these questions and the facts which they rest upon are now forcing to the front. Have we anything definite, drawn from the study of the individual on the psychological side, to substitute for these confessedly vague biological phrases? Spencer gave an answer in a general way long ago to the *second* of these questions, by saying that in consciousness the function of pleasure and pain is just to keep some actions or movements going and to suppress others.

"But as soon as we enquire more closely into the actual working of pleasure and pain reactions, we find an answer suggested to the *first* question also, *i. e.*, the question as to how the organism comes to make the kind and sort of movements which the environment calls for—the *movement variations when and where they are required.* The pleasure or pain produced by a stimulus—and by a movement also, for the utility of movement is always that it secures stimulation of this sort or that—does not lead to diffused, neutral, and characterless movements, as Spencer and Bain suppose; this is disputed no less by the infant's movements than by the actions of unicellular creatures. There are characteristic differences in vital move-

[6] This is just the question that Weismann seeks to answer (in respect to the supply of variations in forms which the paleontologists require), with his doctrine of 'Germinal Selection' (*Monist*, Jan., 1896). Why are not such applications of the principle of natural selection to variations *in the parts and functions of the single organism* just as reasonable and legitimate as it is to variations in separate organisms? As against "germinal selection," however, I may say, that in the cases in which ontogenetic adaptation sets the direction of survival of phylogenetic variations (as held in this paper) the hypothesis of germinal selection is in so far unnecessary. This view finds the operation of selection *on functions in ontogeny* the means of securing "variations when and where they are wanted;" while Weismann supposes competing germinal units.

ments wherever we find them. Even if Mr. Spencer's undifferentiated protoplasmic movements had existed, natural selection would very soon have put an end to it. There is a characteristic antithesis in vital movements always. Healthy, overflowing, outreaching, expansive, vital effects are associated with pleasure; and the contrary, the withdrawing, depressive, contractive, decreasing, vital effects are associated with pain. This is exactly the state of things which the theory of selection of movements from overproduced movements requires, *i. e.*, that increased vitality, represented by pleasure, should give the excess movements, from which new adaptations are selected; and that decreased vitality represented by pain should do the reverse, *i. e.*, draw off energy and suppress movement.[7]

"If, therefore, we say that here is a type of reaction which all vitality shows, we may give it a general descriptive name, *i. e.*, the "Circular Reaction," in that its significance for evolution is that it is not a random response in movement to all stimulations alike, but that it distinguishes in its very form and amount between stimulations which are vitally good and those which are vitally bad, tending to retain the good stimulations and to draw away from and so suppress the bad. The term 'circular' is used to emphasize the way such a reaction tends to keep itself going, over and over, by reproducing the conditions of its own stimulation. It represents habit, since

[7] It is probable that the origin of this antithesis is to be found in the waxing and waning of the nutritive processes. "We find that if by an organism we mean a thing merely of contractility or irritability, whose round of movements is kept up by some kind of nutritive process supplied by the environment—absorption, chemical action of atmospheric oxygen, etc.—and whose existence is threatened by dangers of contact and what not, the first thing to do is to secure a regular supply to the nutritive processes, and to avoid these contacts. But the organism can do nothing but move, as a whole or in some of its parts. So then if one of such creatures is to be fitter than another to survive, it must be the creature which by its movements secures more nutritive processes and avoids more dangerous contacts. But movements toward the source of stimulation keep hold on the stimulation, and movements away from contacts break the contacts, that is all. Nature selects these organisms; how could she do otherwise? . . . . We only have to suppose, then, that the nutritive growth processes are by natural selection drained off in organic expansions, to get the division in movements which represents this earliest bifurcate adaptation." (Ref. 2, p. 201).

it tends to keep up old movements; but it secures new adaptations, since it provides for the overproduction of movement variations for the operation of selection. This kind of selection, since it requires the direct coöperation of the organism itself, I have called 'Organic Selection.'"

The advantages of this view seem to be somewhat as follows:

1. It gives a method of the individual's adaptations of function which is *one in principle with the law of overproduction and survival now so well established in the case of competing organisms.*

2. It reduces nervous and mental evolution to strictly parallel terms. The intelligent use of phylogenetic variations for functional purposes in the way indicated, puts a premium on variations which can be so used, and thus sets phylogenetic progress *in directions of constantly improved mental endowment.* The circular reaction which is the method of intelligent adaptations is liable to variation in a series of complex ways which represent phylogenetically the development of the mental functions known as memory, imagination, conception, thought, etc. We thus reach a phylogeny of mind which proceeds in the direction set by the ontogeny of mind,[8] just as on the organic side the phylogeny of the organism gets its determinate direction from the organism's ontogenetic adaptations. And since it is the one principle of Organic Selection working by *the same functions* to set the direction of both phylogenies, the physical and the mental, the two developments are not two, but one. Evolution is, therefore, not more biological than psychological (ref. 2, chap. x, xi, and especially pp. 383–388).

3. It secures the relation of structure to function required by the principle of "use and disuse" in ontogeny.

4. The only alternative theory of the adaptations of the individual are those of "pure chance," on the one hand, and a "creative act" of consciousness, or the other hand. Pure chance is refuted by all the facts which show that the organism does not wait for chance, but goes right out and effects new adaptations to its environment. Furthermore, ontogenetic

[8] Prof. C. S. Minot suggests to me that the terms "ontopsychic" and "phylopsychic" might be convenient to mark this distinction.

adaptations are determinate; they proceed in definite progressive lines. A short study of the child will disabuse any man, I think, of the "pure chance" theory. But the other theory which holds that consciousness makes adaptations and changes structures directly by its *fiat*, is contradicted by the psychology of voluntary movement (ref. 4, 6, 7). Consciousness can bring about no movement without having first an adequate experience of that movement to serve on occasion as a stimulus to the innervation of the appropriate motor centers. "This point is no longer subject to dispute; for pathological cases show that unless some adequate idea of a former movement made by the same muscles, or by association some other idea which stands for it, can be brought up in mind the intelligence is helpless. Not only can it not make new movements; it can not even repeat old habitual movements. So we may say that intelligent adaptation does not create coördinations; it only makes functional use of coördinations which were alternatively present already in the creature's equipment. Interpreting this in terms of congenital variations, we may say that the variations which the intelligence uses are alternative possibilities of muscular movement" (ref. 4). So the only possible way that a really new movement can be made is *by making the movements already possible so excessively and with so many varieties of combination, etc., that new adaptations may occur.*

5. The problem seems to me to duplicate the conditions which led Darwin to the principle of natural selection. The alternatives before Darwin were "pure chance" or "special creation." The law of "overproduction with survival of the fittest" came as the solution. So in this case. Let us take an example. Every child has to learn how to write. If he depended upon chance movements of his hands he would never learn how to write. But on the other hand, he can not write simply by willing to do so; he might will forever without effecting a "special creation" of muscular movement. What he actually does is to *use his hand in a great many possible ways as near as he can to the way required;* and from these excessively produced movements, and after excessively varied and numerous trials, he gradually selects and fixes the slight successes made

in the direction of correct writing. It is a long and most laborious accumulation of slight Organic Selections from over-produced movements (ref. for handwriting in detail, 2, chap. v; also 2, pp. 373, ff.).

6. The only resort left to the theory that consciousness is some sort of an *actus purus* is to hold that it *directs* brain energies or selects between possible alternatives of movement; but besides the objection that it is as hard to direct movement as it is to make it (for nothing short of a force could release or direct brain energies), we find nothing of the kind necessary. The attention is what determines the particular movement in developed organisms, and the attention is no longer considered an *actus purus* with no brain process accompanying it. The attention is a function of memories, movements, organic experiences. We do not attend to a thing because we have already selected it, or because the attention selects it; but *we select it because we—consciousness and organism—are attending to it.* "It is clear that this doctrine of selection as applied to muscular movement does away with all necessity for holding that consciousness even directs brain energy. The need of such direction seems to me to be as artificial as Darwin showed the need of special creation to be for the teleological adaptations of the different species. This need done away, in this case of supposed directive agency as in that, the question of the relation of consciousness to the brain becomes a metaphysical one, just as that of teleology in nature became a metaphysical one; and it is not to much profit that science meddles with it. And biological as well as psychological science should be glad that it is so, should it not?" (ref. 6; and on the metaphysical question, ref. 7).

V.

A word on the relation of this principle of Organic Selection to Natural Selection. Natural Selection is too often treated as a positive agency. It is not a positive agency; it is entirely negative. It is simply a statement of what occurs when an organism does not have the qualifications necessary to enable it to survive in given conditions of life; it does not in any way

define positively the qualifications which do enable other organisms to survive. Assuming the principle of Natural Selection in any case, and saying that, according to it, if an organism do not have the necessary qualifications it will be killed off, it still remains in that instance to find what the qualifications are which this organism is to have if it is to be kept alive. So we may say that *the means of survival is always an additional question* to the negative statement of the operation of natural selection.

This latter question, of course, the theory of variations aims to answer. The positive qualifications which the organism has arise as congenital variations of a kind which enable the organism to cope with the conditions of life. This is the positive side of Darwinism, as the principle of Natural Selection is the negative side.

Now it is in relation to the theory of variations, and not in relation to that of natural selection, that Organic Selection has its main force. Organic Selection presents *a new qualification of a positive kind* which enables the organism to meet its environment and cope with it, while natural selection remains exactly what it was, the negative law that if the organism does not succeed in living, then it dies, and as such a qualification on the part of the organism, Organic Selection presents several interesting features.

1. If we hold, as has been argued above, that the method of Organic Selection is always the same (that is, that it has a natural method), being always accomplished by a certain typical sort of nervous process (*i. e.*, being always neuro-genetic), then we may ask whether that form of nervous process—and the consciousness which goes with it—may not be a variation appearing early in the phylogenetic series. I have argued elsewhere (ref. 2, pp. 200 ff. and 208 ff.) that this is the most probable view. Organisms that did not have some form of selective response to what was beneficial, as opposed to what was damaging in the environment, could not have developed very far; and as soon as such a variation did appear it would have immediate preëminence. So we have to say either that selective nervous property, with consciousness, is a variation,

or that it is a fundamental endowment of life and part of its final mystery. "The intelligence holds a remarkable place. It is itself, as we have seen, a congenital variation; but it is also the great agent of the individual's personal adaptation both to the physical and to the social environment" (ref. 4).

"The former (instinct) represents a tendency to brain variation in the direction of fixed connections between certain sense-centers and certain groups of coördinated muscles. This tendency is embodied in the white matter and the lower brain centers. The other (intelligence) represents a tendency to variation in the direction of alternative possibilities of connection of the brain centers with the same or similar coördinated muscular groups. This tendency is embodied in the cortex of the hemispheres" (ref. 4).

2. But however that may be, whether ontogenetic adaptation by selective reaction and consciousness be considered a variation or a final aspect of life, it is a *life-qualification of a very extraordinary kind.* It opens a new sphere for the application of the negative principle of natural selection upon organisms, *i. e.*, with reference to *what they can do*, rather than to what they are; to the new use they make of their congenital functions, rather than to the mere possession of the functions (ref. 2, pp. 202 f.). A premium is set on congenital plasticity and adaptability of function rather than on congenital fixity of function; and this adaptability reaches its highest in the intelligence.

3. It opens another field also for the operation of natural selection—still viewed as a negative principle—through the survival of particular overproduced and modified reactions of the organism, by which the determination of the organism's own growth and life-history is secured. If the young chick imitated the old duck instead of the old hen, it would perish; it can only learn those new things which its present equipment will permit—not swimming. So the chick's own possible actions and adaptations in ontogeny have to be selected. We have seen how it may be done by a certain competition of functions with survival of the fit. But this is an application of natural selection. I do not see how Henslow, for example, can get the so-called "self-adaptations"—apart from "special

creation"—which justify an attack on natural selection. Even plants must grow in determinate or "select" directions in order to live.

4. So we may say, finally, that Organic Selection, while itself probably a congenital variation (or original endowment) works to secure new qualifications for the creature's survival; and its very working proceeds by securing a new application of the principle of natural selection to the possible modifications which the organism is capable of undergoing. Romanes says: "it is impossible that heredity can have provided in advance for innovations upon or alterations in its own machinery during the lifetime of a particular individual." To this we are obliged to reply in summing up—as I have done before (ref. 2, p. 220)—we reach "just the state of things which Romanes declares impossible—heredity providing for the modification of its own machinery. Heredity not only leaves the future free for modifications, it also provides a method of life in the operation of which modifications are bound to come."

VI.

The Matter of Terminology.—I anticipate criticism from the fact that several new terms have been used in this paper. Indeed one or two of these terms have already been criticised. I think, however, that novelty in terms is better than ambiguity in meanings. And in each case the new term is intended to mark off a real meaning which no current term seems to express. Taking these terms in turn and attempting to define them, as I have used them, it will be seen whether in each case the special term is justified; if not, I shall be only two glad to abandon it.

Organic Selection.—The process of ontogenetic adaptation considered as keeping single organisms alive and so securing determinate lines of variation in subsequent generations. Organic Selection is, therefore, a general principle of development which is a direct substitute for the Lamarkian factor in most, if not in all instances. If it is really a new factor, then it deserves a new name, however contracted its sphere of application may finally turn out to be. The use of the word

"Organic" in the phrase was suggested from the fact that the organism itself coöperates in the formation of the adaptations which are effected, and also from the fact that, in the results, the organism is itself selected; since those organisms which do not secure the adaptations fall by the principle of natural selection. And the word "Selection" used in the phrase is appropriate for just the same two reasons.

Social Heredity.—The acquisition of functions from the social environment, also considered as a method of determining phylogenetic variations. It is a form of Organic Selection but it deserves a special name because of its special way of operation. It is really heredity, since it influences the direction of phylogenetic variation by keeping socially adaptive creatures alive while others which do not adapt themselves in this way are cut off. It is also heredity since it is a continuous influence from generation to generation. Animals may be kept alive let us say in a given environment by social cooperation only; these transmit this social type of variation to posterity; *thus social adaptation sets the direction of physical phylogeny and physical heredity is determined in part by this factor.* Furthermore the process is all the while, from generation to generation, aided by the continuous chain of extra-organic or purely social transmissions. Here are adequate reasons for marking off this influence with a name.

The other terms I do not care so much about. "Physico-genetic," "neuro-genetic," "psycho-genetic," and their correlatives in "genic," seem to me to be convenient terms to mark distinctions which would involve long sentences without them, besides being self-explanatory. The phrase "circular reaction" has now been welcomed as appropriate by psychologists. "Accommodation" is also current among psychologists as meaning single functional adaptations, especially on the part of consciousness; the biological word "adaptation" refers more, perhaps, to racial or general functions. As between them, however, it does not much matter.[9]

[9] I have already noted in print (ref. 4 and 6) that Prof. Lloyd Morgan and Prof. H. F. Osborn have reached conclusions similar to my main one on Organic Selection. I do not know whether they approve of this name for the "factor;" but as I suggested it in the first edition of my book (April, 1895) and used it earlier, I venture to hope that it may be approved by the biologisst.

THE PATH OF THE WATER CURRENT IN CUCUMBER PLANTS.

By Erwin F. Smith.

(*Continued from page 457*).

3. Downward Movement of One Per Cent Eosine Water in Cut Stems Not Severed From Their Roots.

(No. 17). This was a young vine, 120 centimeters long, full of blossoms and young fruits and very thrifty; it bore about 24 leaves, the largest five averaging 20 cm. in breadth. March 23, 3:20 P. M. The terminal 12 cm. of the stem was cut away under water and the stump bent over and plunged into 1 per cent eosine water. The sun shone hot and the air of the house was rather dry. 4:20 p. m. No trace of stain in the veins of any of the leaves. March 25, noon. It is now over 44 hours since the cut stem was plunged into the eosine water and judging from the quantity remaining in the bottle no measurable volume has gone down the stem. The external appearance, proceeding from above downwards, is as follows: The first internode (the one in the eosine and just above it) is badly shriveled and diffusely stained. The first leaf (9.5 cm. from the cut end) is not quite as turgid as the rest, and its veins show a faint stain. The second internode (10 cm.) is pinkish green and in the grooves of the stem pink, especially toward the upper end, seeming to indicate that most of the stain has passed through the inner ring of bundles. The veins of the second leaf are also distinctly but faintly pink. The petiole of this leaf is 9 cm. long and its blade 12 cm. broad, and the same pale stain is to be seen in all of the veins. Further down there is no external evidence of stain. The downward movement of the stain has, therefore, been very slight. 1:30 p. m. A long tendril from the second node shows a faint internal stain outward for a distance of 10 cm. On cutting, this is seen to be due to stain lodged in the bundles, while at its base there is also a little diffuse stain. The stain now shows

through the interior of the third node which is 9 cm. long. 1:35 p. m. The stem was now cut for examination. The surface of the eosine water in the bottle has not lowered perceptibly. The diffuse stain in the first internode includes everything; the tissues are shriveled and seem to be dead. In the petiole of the first leaf there is a faint stain of the xylem part of each bundle; no diffuse stain into the phloem or any of the tissues outside of the bundle. At the base of the second internode (9 cm farther from the cut stem) the entire xylem of each bundle shows a pale red stain and this has diffused out from three bundles into the surrounding tissues. The second petiole, cut in the middle, shows a faint pink stain, best seen under the lens. It is sharply restricted to the bundles, but occurs in each one and includes the whole of the xylem. At the base of the third internode (9 cm. farther away from the fluid) the stain is fainter and is restricted to the xylem. It is in all of the bundles and is sharper (?) in the spirals of some. Apex of third petiole (down) shows faintest trace of color in 3 bundles, only to be seen under the lens. Color more distinct in the middle part but very faint. Base of fourth internode (9 cm. further from the eosine) there is a very faint stain sharply restricted to the xylem of 6 bundles, all of which is stained. Middle of next lower petiole shows barest trace of stain in two bundles, not visible without a lens. Stain visible in ten bundles of a small fruit from the same node. The base of the next internede (10 cm. further down) shows not a trace of stain. Five cm. farther up, no stain. Additional 3 cm. up, i. e., close under the node, there is a faint stain in the xylem of three bundles and this is not restricted to the spirals. One-half centimeter closer to the node the color is faint and is still restricted to the three bundles.

The stain seems to have travelled in all of the lignified walls, and it appears clear that the spirals did not carry it more than the other woody parts of the bundle. The movement of the eosine water down these stems, contrary to the water current, was scarcely more abundant than the upward movement past the gelatine plugs. Judging from this, the very slow downward movement of the stain apparently follows another

law than that governing the rapid upward movement of the transpiration water, i. e., that of surface tension or capillarity.

(No. 19). This was a large old vine, nearly destitute of leaves, the only large one being 8 centimeters below the cut stem. March 23, 4:06 p. m. The tip of this stem was cut under water and immediately transferred to 1 per cent eosine water. 4:15 p. m. No stain in the veins of the first leaf, 8 cm. from the cut. March 25, 12:45 p. m. The leaf, 8 cm. from the cut end, is flabby and its veins show a very decided stain. Farther down there is no stain visible externally. The stem was now removed from the fluid and cut open for examination. At 5 cm. down there was a diffuse stain involving the whole stem, but it was not dense and the bundles were not deeper stained than farther down the stem. At 10 cm. the sieve tube tissue was stained as well as the xylem and there was also a slight diffuse stain into the parenchyma, but the general tone of the stem remained green. At 20 cm. from the cut tip one of the 9 bundles (outer ring) showed no stain. No stain outside of the bundles. At 40 cm. from the cut all of the bundles showed the stain but in one (outer ring) it was much fainter than in the rest. The color was a decided pale red, including the whole of the xylem but not extending to any other part of the stem. At 80 cm. down, the stain was restricted to 4 bundles (the whole of the xylem part) and was barely discernable. At 85 cm. there was still a trace in these bundles—stain in the whole of the xylem and not brighter in the spirals. At 90 centimeters, and farther down, the stain was wholly absent.

This also proved a very instructive stem. The fact that at remote distances the stain was not restricted to the spiral vessels of the stem but tinged the whole xylem equally (the lignified walls) is very striking and decidedly different from the results obtained by passing the stain up the stem, in which case the spirals are stained ahead of the pitted vessels and are clearly seen to be the carriers of the eosine. In this case that portion of the stem in the fluid was not shriveled, probably because it was old and woody.

4. Movement of Water Through Boiled Stems Not Severed From the Plant.

(No. 11). A fine thrifty vine, 180 centimeters long, bearing 18 large leaves and half as many more small ones. The largest leaves have a spread of 17 to 19 centimeters. March 21, 4:00 p. m. About 35 cm. from the earth, the bright green stem was bent over and immersed for a distance of 20 cm. in hot water. An attempt was made to boil this water but the heat under the basin was not sufficient, although ample to kill the stem. 4:30 p. m. The temperature of the water during the last half hour has risen from 71° C. to 75° C. There is no change in the color of the immersed part of the stem, nor any change in the foliage above, but the effect of the hot water is already noticeable in the very decided shrinkage of the immersed stem. It has shrunk in diameter nearly one-half. 4:50 p. m. During the last 20 minutes the temperature of the water has risen only one degree. This was now poured out and water at 89° C. substituted. In pouring, the temperature fell to 85° C. In this hotter water the stem quickly became paler green. 4:58 p. m. Temp. of water 80° C. The immersed part of the stem has now shrunk to one-third of its normal diameter, and this shrinkage has extended both up and down, for a short distance out of the water (a few centimeters). 5:15 p. m. Temp. now down to 76° C. Stem taken out. Except the apex of one leaf, 15 cm. up, the foliage did not become flabby. Below the boiled part is a small branch with half a dozen leaves, sufficient to carry the roots. March 22, 11 a. m. The boiled part of the stem, which is now dry and greenish-brown, was wrapped in many folds of rubber cloth. The foliage of this vine shows no wilt, except parts of 5 small leaves, which were near the boiled part and may have been injured by the heat of the lamp. It is windy and sunny and the air of the house is rather dry so that transpiration is active. Temperature in shade, 1 foot above the bench, 26° C. Noon. A check vine (cut off at base, yesterday p. m.) has wilted and shriveled. Temperature three feet above the bench, among the leaves, 30° C. 1:20 p. m. No change. What is

especially surprising is that the tender terminal leaves show no signs of wilt. 4:15 p. m. This vine has stood up remarkably to-day. The transpiration demands have been large and there has been no wilt—not a trace—that mentioned as occurring on a few of the small basal leaves being evidently due to imperfect protection from the heat of the lamp when the stem was boiled. March 23, 11 a. m. Sunny and hot; some wind; air of the house rather dry, and transpiration large. No wilt of the foliage except the margins and tips of the blades of three big leaves midway up the stem. These are slowly drying out. 12:30 p. m. The greater part of the foliage on this vine is still turgid and normal in appearance. The tips and margins of the three leaves above mentioned are crisp, but this injury involves only a small part of each leaf. Transpiration active. Temp. in sun 30° C. Dry bulb 26.5° C.; wet bulb 22° C. 3:00 p. m. Slight, if any, change. Nearly all of the leaves are turgid and entirely normal in appearance, including all at the top of the vine. 4:20 p. m. No change since the last record. The vine stands up well. Temp. now 24° C. Active transpiration all day. March 25, 1:15 p. m. The vine stands up well. Nearly all of it is perfectly healthy, including the tender upper part, but portions of the lower leaves already mentioned are slowly drying out and in a very interesting manner, i. e., after the fashion of the California vine disease, the larger veins and their branches and a little of the adjacent parenchyma remaining green, even dark green, while the parenchymatic areas between the veins, especially at the apex of the blades and on the margins, are becoming first yellow and then a dead brown. 5:30 p. m. Vine stands up beautifully. It is four days since the stem was killed by the hot water. March 26, 2:45 p. m. A great change for the worse since yesterday. All of the foliage has now wilted (as yet only the blades) and the large leaves midway down as well as the smaller lower ones are rapidly drying out. March 27, 1:20 p. m. All of the leaves are now crisp, except a few very small flabby ones which are in the vicinity of a half grown fruit from which they are drawing water. The stem is still turgid but some of the petioles begin to droop.

The leaves below the boiled part are still healthy. March 28, 1:30 p. m. The stem and the petioles are still green but the latter are becoming more and more flabby, most of them at the top of the vine having lost all of their turgor.

This vine was able to draw all the transpiration water necessary to supply a large leaf surface (more than 3.000 sq. cm.) through about 25 centimeters of dead stem for a period of four days, during a part of which time the transpiration was very active. All of this water must have passed up through the bundles, since all the outer parts were dead and dry and shriveled down onto the bundles, the vessels of which preserved their shape unaltered as shown by subsequent examination.

(No. 13). This vine was 130 centimeters long. It bore six small leaves and 12 large ones, the best averaging 17 cm. in breadth. March 22, 1:02 p. m. The stem was bent over near the earth and inserted for a distance of 18 centimeters into water at 90° C. In two minutes the temperature rose to 95° C. 1:07 p. m. Water simmering; temp. 97° C. Boiled part not yet noticeably smaller. 1:10 p. m. Stem shows shrinkage and change of color. 1:15 p. m. Slight loss of turgidity in most of the leaves. 1:20 p. m. A marked shrinkage of the diameter of the stem is now first visible. The flabbiness of the foliage is increasing rapidly, every leaf is affected. 1:27 p. m. Water has remained at 97° since last record. Stem taken out because of the marked wilt of the foliage. This wilt appears to be due to the transpiration of hot water. The wilt is too sudden and decided to be due to anything else. The stem has not only shriveled in the water but also for a distance of 10 cm. up and 5 cm. below, making a total of 33 cm. of dead stem. Sun hot; earth and air of house rather dry; transpiration active. Such an experiment were better tried when the air is nearly saturated and transpiration slight. 1:45 p. m. Stem wrapped in many folds of rubber cloth. Roughly estimated it has shrunk to about one-third its normal diameter. The leaves seem to be recovering their turgor.

2:00 p. m. The lowest leaves are still flaccid but the upper ones have fully regained their turgor. 3:45 p. m. The lower leaves have now also regained their turgor. Its loss was

clearly due to the transpiration of hot water. (Subsequent experiments showed that it is very easy to push this wilting beyond the power of the plant to recover). 4:15 p. m. The plant stands up well. There is no trace of wilt. March 23, 11 a. m. No sign of wilt. Noon. The lowest five leaves show distinct signs of wilt at the tip of the blade. None of the upper leaves show any trace of it. 12:25 p. m. The wilting is worse but is still confined to the lower leaves. It is very decided on the lowest one which is exposed to the bright sun. The tender apical leaves are turgid, as well as those in the mid part of the stem. 1:20 p. m. The leaf next to the lowest one begins to crisp. 3:00 p. m. Blade of lowest leaf but one is now crisp, and the blades of the other four are drying out at the apex and on the margins and between the larger veins. 4:30 p.•m. No change. The bulk of the foliage stands up well, including all of the upper leaves March 25, 1:20 p. m. The lower leaves of this plant are dried out to a greater extent than are those of No. 11, but the major part of the foliage is normal and the tips of both vines are noticeably turgid. The drying out of the parenchyma between the veins is also to be seen in the affected leaves of this vine, the larger veins and a narrow border of the leaf parenchyma remaining a bright green. 5:40 p. m. The vine stands up well. It is three days and four hours since the stem was boiled. March 26, 3:00 p. m. The vine begins to show symptoms of collapsing. All of the petioles are turgid, but the blade of the lowest leaf is nearly dry, that of the next up is wholly dry; those of the next three above are crisp at the apex and on the mar gins (one-fifth to one-third the surface); the three next up show a trace of drying on their margins, and in all the rest there is a faint suggestion of loss of turgor. March 27, 2:00 p. m. All of the leaves on this vine are now crisp-dry except three at the top which are flabby. The stem and the petioles are still turgid. March 28, 1:30 p. m. The upper three leaves are still flabby, and all of the petioles are still rigid except the tips of some of the lower ones which begin to droop.

This vine gives results confirmatory of the preceding. For more than three days the plant was able to draw all of the

water necessary for its use through 33 cm. of dead stem. Probably if air could be prevented from gradually passing through the shriveled stem into these water carrying vessels and interfering with the normal condition of things the plant might continue to draw its water through a dead stem almost indefinitely.[1]

5. The Result of Parasitic Plugging of the Vessels.

From these experiments and those upon the cucumber wilt, which I have published elsewhere, it follows that the downward path of *Bacillus tracheiphilus* from the inoculated leaf blade into the stem of the cucumber (for an account of this disease see *Centr. f. Bakt. u. Par. Allg.* I, No. 9–10, 1895) is exactly that made use of by the ascending water current, just as I stated it to be at the Brooklyn meeting of the A. A. A. S., and the general wilt of the foliage may be explained, first, by a functional disturbance, due to the more or less complete clogging of the lumina of the spiral vessels with countless millions of these bacteria which thrive in the alkaline fluid of the vessels, and, second, by a structural disturbance, due to the breaking down (dissolving) of the walls of these spirals and the flooding out and subsequent growth of the bacteria in the surrounding parenchyma and in the pitted vessels, accompanied, of course, by the more or less free entrance of air into the spirals. It is probable, although not enough examinations have yet been made to render this certain, that no leaf wilts from secondary infection until the water carrying spirals in its petiole have become clogged by the bacillus, i. e., that the wilt of the leaf is not induced by the partial clogging of the vessels farther down in the stem. This is the more likely, first, from the fact that there is always a progressive wilt, leaf after leaf, beginning with the ones nearest the point of infection and moving both ways therefrom, and, second, from the fact that very rarely are all of the pitted vessels filled, so that water lifted up from the roots has always the opportunity to

[1] Those who wish to follow these subjects may consult the above mentioned work by Strasburger, pp. 510–936, where many interesting experiments are detailed.

pass around the clog in the spirals by way of the unfilled pitted vessels and to enter the spirals once more farther up. Were this not so, i. e., were pitted vessels filled as readily, as quickly, and as fully as the spirals, we should have not the gradual wilt of leaf after leaf up and down the stem, but the sudden collapse of all the leaves beyond the original point of attack. This is exactly what does happen in watermelon vines attacked by *Fusarium niveum*, (for a brief account of this parasite see *Proc. Am. Asso. Adv. Sci.*, Vol. 43, 1894, p. 289, and *Ibid*, Vol. 44, 1895, p.) where the pitted vessels appear to fill with the fungus as soon, if not sooner, than the spirals.

These two diseases of cucurbits are very interesting from a physiological standpoint, and both parasites lend themselves readily to infection experiments, their slightly different behavior being, perhaps, accounted for by the fact that the fungus is strictly ærobic, while the bacillus is facultative anærobic. Whatever be thought of butter or gelatine, it certainly cannot be maintained that the mere presence of these parasites in the lumina of the vessels destroys the carrying capacity of the uninjured walls, and yet they act quite as effectually as gelatine, paraffin, or cocoa butter plugs, causing, when they fill the vessels only incompletely, a flabbiness of the foliage, which is proportionate to the extent of the plugging and to the activity of the transpiration, and which may give place to complete turgor in periods when the transpiration is small (night, early morning, or damp days), and producing, when they completely fill the lumina of the vessels, an entire collapse of the foliage, from which there is no recovery. In case of the cucumber this collapse takes place as soon as the spiral vessels leading into any petiole are filled by the bacillus.

EDITOR'S TABLE.

—Professors in the scientific departments of our schools should exercise their influence to prevent the spoliation of nature that is going on at so rapid a rate in our country. We do not especially refer at present to forest fires which involve so much financial loss that our state and general governments are moving in the direction of their prevention. In passing, however, we must refer to the railroad companies as delinquents in this matter, and insist that heavy fines be imposed on them in all cases where fires can be shown to have originated from locomotives. We counted from the car windows of a train not long since, twelve distinct fires burning near the track in the space of a few miles, in a forest covered region not far from Philadelphia, and no one appeared to pay any attention to them.

We wish, however, to refer to the destruction wrought near our cities by the uprooting of plants and the breaking off of branches for purposes of decoration of public and private houses. Within reasonable bounds the vegetable world furnishes material for such decoration, but the practice is carried beyond the rich resources of nature to meet. Our woods are being rapidly stripped of ornamental plants for miles all round our large cities. In many regions the *Epigæa repens* is completely destroyed, and the blooms of the dogwood and kalmia no longer appear. Lycopodia are uprooted over large tracts, and must now be brought from considerable distances. Some of the ruin is wrought for church decoration, and the girl-graduate is responsible for more of it. Teachers of the natural sciences can teach their hearers that this cannot go on forever. Especially can they point out that botanical classes should not gather arm-loads of orchids of fastidious habits if they do not wish to see the localities destroyed or the species well nigh exterminated.

The authorities in charge of our public parks might, in some places, profitably change their point of view. A park should not consist principally of graded paths lined with stone curbs or walls, separated by tracts of close shorn grass. Shrubberies of nature's planting should remain, and the vines with which nature festoons the forest should not be cut down. No harm is done if there are places where rabbits may hide, and wild birds may nest. Even an owl or two might be permitted to keep down so far as he or she can, the English Sparrow nuisance. In fact, a park is not necessarily a place from which nature is

excluded. The perpetual clearing of undergrowth means also the ultimate destruction of forest, as the natural succession is thus prevented.

As an offset to this public and private vandalism, we have near our cities a goodly number of citizens who preserve more or less of nature in their private parks. It will be to these to whom we must look to replenish our stock of native shrubs and herbs, if the vandal continues to have full swing elsewhere.

THE forty-fifth meeting of the American Association for the Advancement of Science to commence at Buffalo, N. Y., on August 22d, will be characterized by one feature which is deemed by the society an improvement over previous meetings. No excursions will be made during the working hours of the day during the session, only those occupying evening hours being acceptable. At the close of the meeting the field for such diversions will be clear. The geological excursions have been so arranged as not to conflict with the meetings; and the six scientific societies, which meet about the same time, it is hoped will contribute to the importance of the general gathering. It is anticipated that these arrangements will arrest the tendency to dissipation of energy which has been apparent during the last few years. If the habit of many of the embryologists to absent themselves could be overcome, the full force of the Association would be represented. It is expected that a number of evening lectures will present to the public the latest results of research in America.

RECENT LITERATURE.

Surface Colors:—The object of the little book on this subject[1] by Dr. Walter, of Hamburg is apparently to furnish zoologists, mineralogists, and chemists with an accurate explanation of certain color phenomena which are not as yet universally understood, and which are incompletely treated even in the best text-books on Physics. The keynote of the whole book is given in a single sentence of the introductory chapter. "The intensity of the light reflected from any body may be calculated by Fresnel's ordinary formulæ for colorless substances, in the case of those rays which are slightly or not at all absorbed by the

[1] Die Oberflächen-oder Schillerfarben, von Dr. B. Walter, pp. VIII + 122, Braunschweig, F. Vieweg und Sohn, 1895.

body in question; but for wave-lengths which are strongly absorbed by the given substance, Cauchy's formulæ for the intensity of metallic reflection should be used." It appears from these formulæ that the intensity of the reflected light depends on the index of refraction and on the coëfficient of absorption of the substance presenting the reflecting surface. Since both these factors are different for light of different colors, it is shown that white light must be reflected with some of its "components" relatively weaker than others, *i. e.*, no longer in the proper proportion to give the sensation of white light. The application to the colors seen in the mineral kingdom is illustrated by the example of magnesium cyanplatinite, $Mg\,Pt\,(CN)_4$, where,—as is true of most crystals,—the index of refraction and the coëfficient of absorption vary with the direction in which the light vibrates, as well as with the wave-length of the light. The extent to which true surface color is observable on minerals is not indicated, though the *possibility* of a very wide application is clearly shown.

In the appendices, certain mathematical aspects of the subject are treated in a manner suited to the requirements of physicists.—A. C. G.

The Whence and Whither of Man.[2]—This book comprises a series of lectures dilivered at Union Theological Seminary, with some additional matter. The author discusses the doctrine of Evolution from the standpoint of a theologian. He endeavors to show that the great law of animal and human development as revealed in the sequence of physical and mental development is that those species survive which are best conformed to their environment; that this law holds good in the development of the rational, the dominant faculty in man; and finally, to become higher man he must develop a moral-nature by attaining a knowledge of himself as a moral agent, and while not disregarding the body, he must subordinate its appetites to the higher motives furnished by right and duty. It is in following this line of thought that the author hopes for a definite answer as to the future destiny of man.

The closing chapter deals with the present aspects of the theory of evolution. He here compares the various hypotheses of evolution and considers their merits. He judiciously selects the good elements of all of them, concluding that "each theory contains important truth." He concludes that Nägeli's view of "initial tendencies" is too often undervalued. "My own conviction is steadily strengthening that without

[2] The Whence and Whither of Man. By John M. Tyler, New York, 1896, Charles Scribner's Sons, Publishers.

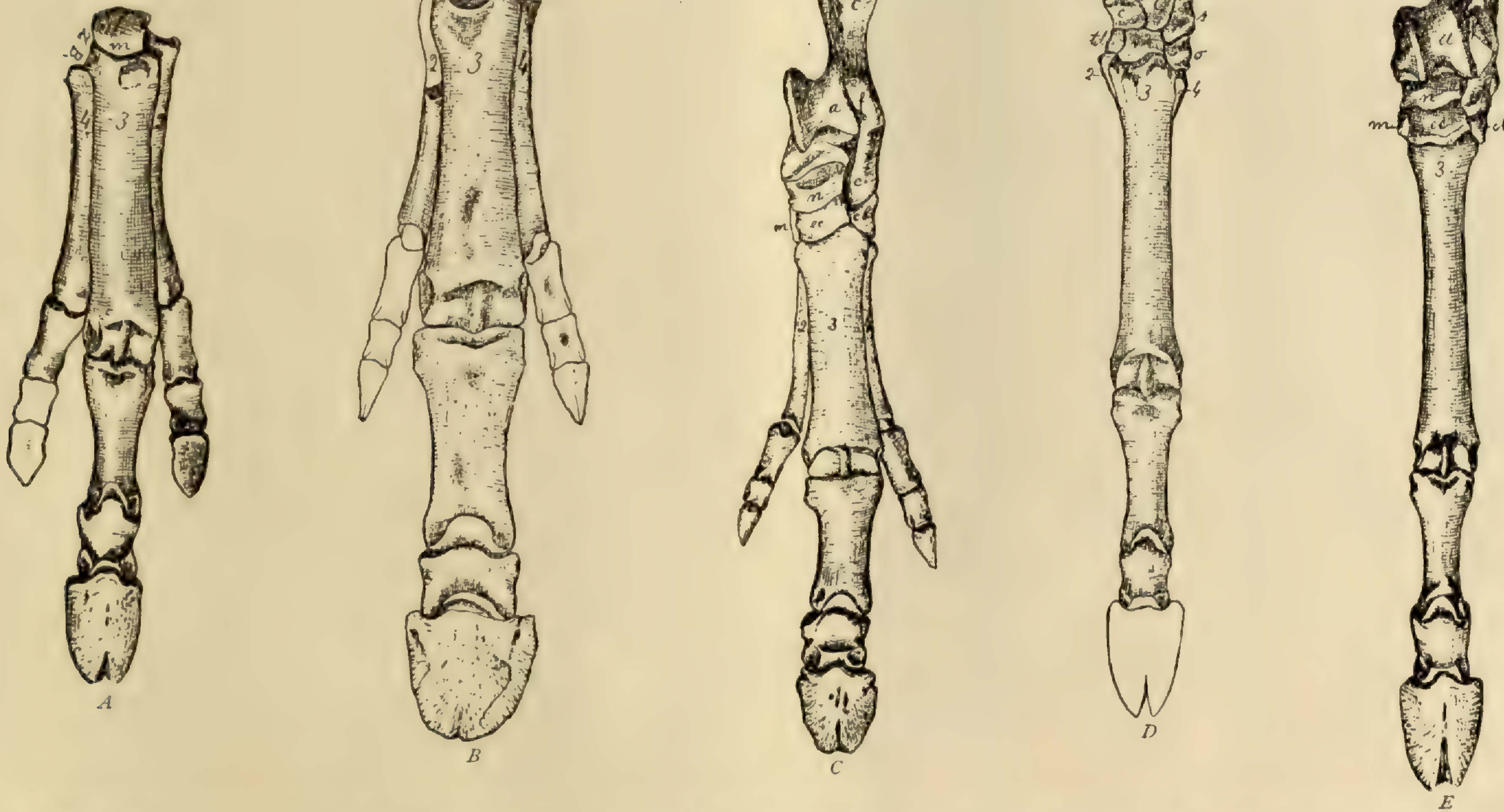
some such original tendency or aim, evolution would never have reached it present culmination in man." He quotes Boveri that "there is too much intelligence in nature for any purely mechanical theory to be possible." It is curious that these authors do not perceive that the sensation of protoplasm, (consciousness), furnishes the basis for the exhibition of the intelligence which they observe, and which has itself undergone evolution coincidentally with the organism. Both orthodox and heterodox evolutionists (theologically speaking) seem equally slow to adopt this view.

Prof. Tyler's book is eminently moderate and reasonable, and will introduce evolution to a large class of readers in an agreeable form.

Cope on the Factors of Organic Evolution.[3]—This book is divided into three parts: I, The nature of variation; II, The causes of variation; III, The inheritance of variation. In the first part it is endeavored to show that variation is not promiscuous or multifarious, but pursues direct courses towards definite ends. This is done by presenting the variations of existing species as to color and structure, and by an examination of the series presented by the forms of vertebrate life in past geologic ages. The latter presentation is a general phylogeny of the vertebrata, with special sections on that of the horse and that of man. The second part is divided into chapters which deal with the physical energies as causes of variation, and the effects of molar motion as seen in variation. These methods of evolution are termed respectively physiogenesis and kinetogenesis. Especial attention is given to kinetogenesis in connection with the phylogeny of vertebrates, since it is in these two fields that most of the original work of the author has been done. The author has demonstrated that the primary cause which has moulded the vertebrate skeleton is molar motion. In the third part, the inheritance of the characters so produced is shown to be the rule, thus demonstrating the inheritance of acquired characters. Theories of inheritance are discussed, and that one which asserts the transmission of energies to the germ plasma is defended. These energies are believed to be the results of a composition between inherited and acquired energies, the whole of them being referred to a class distinct from the inorganic energies, which he has named Bathmic. The last chapter in this part is devoted to a consideration of the relation of consciousness to movements, and hence as a cause

[3] The Primary Factors of Organic Evolution, by E. D. Cope, Professor of Zoology and Comparative Anatomy in the University of Pennsylvania. Chicago: Open Court Pub. Co., Feb., 1896, $2.00.

PLATE X.



Feet of Proterotheriidae from Ameghino. A, fore foot of *Proterotherium cavum* Amegh. B-C, Fore and hind feet of *Diadiaphorus majusculus* Amegh. D-E, Fore and hind feet of *Thoatherium crepidatum* Amegh.

of progressive evolution. The author holds that sensation is a cause of effects which would not appear in its absence, and that its presence conditions progressive evolution. The author holds this to be proven not only by the direct effect of consciousness as observed, but also on the other ground that there is no sufficiency in the inorganic and unconscious organic energies to effect progressive evolution. This is because the well-known tendency of the latter is to the integration of matter and the dissipation of energy, which leads always away from vital phenomena. The author believes the entire vegetable kingdom to be degenerate, its vitality being the expression of automatic energy which derived its self-sustaining character from ancestors endowed with sensation which occupied a position between animals and plants. The Mycetozoa he believes to be existing near relatives of these types.

The book is illustrated by 120 plates and cuts. One of these illustrative of homoplassy, we extract from the chapter on kinetogenesis, with the following explanatory remarks:

"Before reviewing the subject, I cite what is the most remarkable example of homoplassy in the Mammalia which has yet come to the knowledge of paleontologists. Ameghino has discovered in the cenozoic formations of Argentina a group of Ungulata which he calls the Litopterna, and which I regard as a suborder of the Taxeopoda, allied to the Condylarthra (p. 128). Ameghino placed the group under the Perissodactyla, but the tarsus and carpus are of a totally different character, and indicate an origin from the Condylarthra quite independent of that division. The carpal and tarsal bones are in linear series, or if they may overlap, it is in a direction the opposite of that which characterizes the order Diplarthra (=Perissodactyla and Artiodactyla). But the Litopterna present a most remarkable parallelism to the Perissodactyla in the characters of both the feet and the dentition. No genus is known as yet which possesses more than three toes before and behind, and these are of equal length (Macrauchenia Owen). In this genus the teeth are not primitive, but are much modified. The most primitive dentition is seen in the genus Proterotherium (Ameghino) where the superior molars are tritubercular, as in many Condylanthra. In this genus (Pl. X, fig. A) there are three toes, but the lateral ones are reduced, about as in the equine genus Anchitherium (p. 148). In the next genus, Diadiaphorus Amegh., the superior molars are quadritubercular and crested, while the lateral toes are reduced still more, being quite rudimental (figs. B C), as in the equine genera Hippotherium and Prothippus. The superior molars have not progressed so far as in these genera, but are not very different from those of

Anchitherium. In the third and last type (Thoatherium Amegh.) the lateral digits have disappeared from both fore and hind feet (figs. C D), so that the condition is that of the genus Equus (fig. 81), but the splints in the *Thoatherium crepidatum* Amegh. are even more reduced in the known species of horse. The superior molars have not assumed the pattern of the genus Equus, but resemble rather those of Macrauchenia, and could have been easily derived from those of Diadiaphorus.

Here we have a serial reduction of the lateral digits and their connections with the leg, and increase in the proportions of the middle digit and corresponding increases in the proximal connections, exactly similar to that which took place in the horse line, in a different order of Mammalia."

The publishers have done their work well, and are especially to be commended for having made the book of a convenient size to be carried in the pocket or satchel.

The Child and Childhood in Folk-Thought.—(The Child in Primitive Culture); by A. F. Chamberlain; New York, Macmillan & Co., and London, 1896. Pp. x and 464; with bibliography and three indexes; price $3.

Dr. Chamberlain's work is not, as its chief title might lead one to suppose, a mere collection of folk-lore about the child. It is rather an attempt by this means to study the position of the child in primitive society. The author has brought together a great mass of material from every hand, and arranged it systematically under appropriate headings; as a result we find every phase and aspect of childhood represented in his book.

The opening chapters, on the Lore of Motherhood and Fatherhood, have in some places only a remote bearing upon the main topic, but they may be regarded in the light of a general introduction. Following these are a number of chapters which aim to show the attitude of society toward the child; folk-lore on the soul of the child, legends connecting children with animals or plants, stock answers of the adult to the child's questions, superstitions concerning children, etc., together with stories of education and training among uncultured races. A large part of the work deals with the influence of the child upon society —the effect of child-language in modifying adult language; the child's position in many tribes as oracle, judge, physician, or priest, etc. The final chapters are a selection of popular proverbs and sayings bearing upon childhood, from the literature of various races, cultured as well as

uncultured. The bibliography at the end is thorough, if not exhaustive; it consists of over 550 titles, covering the entire field.

The author claims no originality of investigation; but he has culled his material from a host of authorities, and his selections are well made. He has no conclusions to draw; he simply presents the material as data, with a view to a complete survey of the subject. The chief criticism that can be made upon his method is that it frequently leads to a curious intermingling of fables and traditions with actual race customs. Thus in the chapter on the Children's Food is described (p. 150) the practice which holds among several tribes of placing food on the grave of a dead child, to refresh its soul on the way to the spirit-land, and almost immediately after follows the legend of how the infant Hercules obtained immortality. The book is exceedingly interesting; it treats its subject as thoroughly as the breadth of the task together with the limits of the volume permit; and it is wonderfully conducive to further reading.—H. C. Warren.

Stockham on the Ethics of Marriage.[4]—This book is written with the view of securing an excellent object, the increase of the happiness of marriage. As the authoress is an M. D., and as she treats the subject at the outset with a seeming respect for scientific truth, we anticipated something valuable from her point of view. But we are compelled to say that the grains of truth are overlaid with such a quantity of error, rhapsody and sheer silliness, that we can only recommend the book as a study in feminine psychology. That there is one element of common sense running through it we are glad to admit. The authoress sees nothing degrading or indecent in the sexual relation. For this we must praise her; but it was surely not necessary for her to apologize for her good sense, by pages on pages of religious rhapsody. The gist of her method of promoting marital happiness is that sexual intimacy may take place without completing the act. This proposition is as old as the rational faculty of man; but, as rationality is usually less directed to sexual subjects than to any other, it is quite possible that her advice on this point may do some good. There are some amusing passages. Fearing to appear to fall into the Charybdis of "hedonism" she runs high and dry on Scylla, as follows: "Before and during the time some devotional exercises may be participated in, or there may be a formation of consecration of an uplifting character in which both unite!"

[4] Karezza; Ethics of Marriage, by Alice B. Stockham, M. D., Chicago. A. B. Stockham & Co.

The authoress labors under several physiological errors, which should be pointed out. She thinks in common with the ignorant classes generally, that the orgasm is concerned in impregnation, which is well known not to be the case. She also asserts that the secretion of the testis is produced at the time it is needed for use, an idea promulgated several years ago in a silly book called Diana. This is also untrue; its elaboration requires some days, and when the gland is full the secretion makes its presence known and demands expulsion. The present book should have stated also, that the practice she recommends, which she calls "Karezza," is a most potent stimulant of the secretion in question, and does in some men produce enlargement of the prostate gland and orchitis, so that every man must be in this matter his own doctor. But one will not find logic in this book. In view of what precedes one wonders where the authoress got her degree of M. D., and who is responsible for her education. We must, however, once more commend the spirit of the book, and hope that she will be instrumental in teaching some men and women ordinary temperance. But it must be borne in mind that medical writers chiefly deal with pathological conditions, and that the persons she writes about are mostly abnormal through excess or deficiency.

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General Notes.

MINERALOGY.[1]

Contact Goniometer with two Graduated Circles.—In pursuance of the idea already applied to the reflection goniometer (ref. in this journal, 1895, p. 266) Goldschmidt[2] has designed a contact goniometer with two graduated circles. The horizontal circle carries the support for the crystal, which can thus be rotated about a vertical axis. The vertical circle is a metallic band carrying a moveable block. Through the block a small metal rod passes radially toward the center, and on the inner end of the rod a small plate is fixed. By movement of the crystal about its vertical axis and of the block on its arc, the plate may be brought to parallelism with any face on the upper side of the crystal, Actual contact of the plate with the crystal face is effected by sliding the rod through its block. Readings on the two circles give data for computing the position of a plane, exactly as in the case of the reflection goniometer to which reference was above made.

Crystallographic Properties of the Sulphonic Acid Derivatives of Camphor.—About 17 of these compounds are mentioned by Kipping and Pope[3] with much detailed information concerning the crystallograpy of several of them. As might be expected from the fact that the solutions of many of these substances exhibit the phenomenon of circular polarization, the crystals furnish examples of a number of the less common low symmetry grades. Among these are hemimorphism in the monoclinic system (sphenoidal class of Groth), sphenoidal hemihedrism in the orthorhombic system (bisphenoidal class), and probably hemihedrism in the triclinic system (pedial class). Such crystallographic studies must be of great value to stereo-chemistry.

Optical Properties of Lithiophilite and Triphilite.—On these two minerals Penfield and Pratt[4] have based an interesting investigation of the change of optical properties due to the mutual replacement of manganese and iron in isomorphous mixture. It is found

[1] Edited by A. C. Gill, Cornell University, Ithaca, N. Y.
[2] Zeitschr. f. Kryst., XXV, p. 321, 1895.
[3] Zeitschr. f. Kryst., XXV, pp. 225–256, 1895.
[4] Am. Jour. Sci., L, pp. 387–390, Nov., 1895.

that with increasing percentage of iron the index of refraction increases, while the plane of the optical axes is changed from the base (001) to the macropinacoid (100). A specimen containing 26.58% FeO shows an optical angle of 21° 53′ in the basal plane for thallium light, is *uniaxial* for sodium light, and has an angle of 15° 3′ in the macropinacoid for lithium—a remarkably good example of orthorhombic dispersion. With 35.05% FeO the crystals are found to be negative, whereas those with less iron are optically positive. It is suggested that in the pure manganese molecule, the change may be found so great that the brachypinacoid is the plane of the optical axes.

Native Sulphur in Michigan.—Scherzer[5] reports an occurrence of sulphur a mile west of Scofield, Monroe Co., Michigan. It is found in a stratum of impure cavernous limestone about one to three feet in thickness. The pockets, varying from a fraction of an inch up to three feet in diameter, are often lined with calcite and celestite crystals with bright lustrous masses of sulphur toward the center. The removal of about an acre of this bed has yielded 100 barrels of pure sulphur. The sulphur seems to have originated from hydrogen sulphide which is abundant in the waters of the neighborhood. The hydrogen sulphide, in turn, may be a product of decomposing organic matter.

Leadhillite Pseudomorphs at Granby, Mo.—The occurrence of leadhillite at Granby in the form of pseudomorphs after calcite and galena is made the subject of a note by Foote.[6] Scalenohedrons in a chert calamine rock are composed usually of pure cerussite; more rarely the substance is found to be leadhillite. Galena cubes replaced by leadhillite were also observed. In these cases the secondary mineral is usually mixed with remnants of the original galena, producing a "gray amorphous mass." In a few specimens the leadhillite is pure.

Celestite from Giershagen.—According to Arzruni and Thaddéef[7] the axial ratio of "normal" celestite is a : b : c = .78093 : 1 : 1.28324. The mineral from Giershagen, which appears to be chemically pure $Sr\,SO_4$, has the ratio a : b : c = .77962 : 1 : 1.28533. The mean of four determinations places the specific gravity at 3.9665. The optical angle of "normal" celestite is given as $2\,V_{a\,Na} = 50°\ 34'$. This investigation adds another to the list of chemically pure compounds whose

[5] Am. Jour. Sci., L, pp. 246–248, Sept., 1895.
[6] Am. Jour. Sci., L, p. 99, August, 1895.
[7] Zeitschr. f. Kryst., XXV, pp. 38–72, 1895.

molecular volume may be considered as accurately known, and allows of comparison between the various physical constants of this and isomorphous substances.

Minerals from the Galena Limestone.—Hobbs[8] gives a detailed description, with many drawings, of the crystallized minerals from the galena limestone of southern Wisconsin and northern Illinois. The habitus of the various crystals is made prominent in the discussion of them. New forms are reported on calcite $(24.0.\bar{2}4.1)$, on cerussite (0.25.4), and on azurite (307), (203), $(\bar{2}05)$ and (9.12.8).

Miscellaneous Notes.—Becke[9] shows that the center of symmetry may be used as a fundamental conception in developing the 32 classes of crystal symmetry, notwithstanding the fact of its abandonment by Groth and Fedorow.—Sylvite from Stassfurt, investigated by Schimpff[10] with special reference to the impurities of the same, gave K Cl 99.239, Na Cl .242, $Mg\ Cl_2$ .089, $Ca\ SO_4$ .073, H_2S .0023, residue .108, loss on melting .2847. The foreign substances seem to occur chiefly as inclusions with the mother liquor. These figures doubtless give a very good idea of the amount of impurity present, but the extreme right hand digits must be looked upon as mathematics rather than chemistry.—Igelström[11] finds molybdenum, probably present as Mo_2O_3, in the hematite from the "Sjögrube," Gouv. Örebro, Sweden. One specimen of the same material showed spectroscopically the presence of thallium.—Niven[12] notes the discovery on New York Island of numerous interesting specimens of the rare earth minerals xenotine and monazite. Titanite, epidote, beryl and menaccanite are also mentioned.—The mineral named schneebergite by Brezina[13] on the basis of an apparently faulty qualitative investigation is shown by Eakle and Muthmann[14] to be in reality a very pure lime-iron garnet, or topazolite, instead of a calcium antimonite. The specific gravity is 3.838, and the chemical composition:

[8] Zeitschr. f. Kryst., XXV, pp. 257-275, 1895.

[9] Zeitschr. f. Kryst., XXV, pp. 73–78, 1895.

[10] Zeitschr. f. Kryst., XXV, p. 92, 1895.

[11] Zeitschr. f. Kryst., XXV, p. 94, 1895.

[12] Am. Jour. Sci., L, p. 75, July, 1895.

[13] Vehr. d. k. k. geol. Reichsanstalt, 1880, p. 313.

[14] Zeitschr. f. Kryst., XXV, pp. 244–246, 1895.

	found		calculated for $Ca_3 Fe_2 Si_3O_{12}$
SiO_2		35.45	35.43
Fe_2O_3	32.33	32.11	31.50
CaO	32.58		33.07

—Foote[15] gives some details concerning a new mineral which he proposes to name Northupite. It was found by Mr. Northup in the "tailings" from a boring made at Borax Lake, Cal. The crystals are regular octahedrons reaching rarely 1 cm. in diameter. The substance seems to be a double chloride and carbonate of sodium and magnesium. Cleavage imperfect, H = 3.5 to 4.

PETROGRAPHY.[1]

Volcanic Rocks and Tuffs in Prussia.—In the hills east of Ebsdorf, near Marburg, Prussia, are large areas covered by basalt flows, flows of dolerite, and others of rocks intermediate in character between these two, both of which are pre-Tertiary in age, or at any rate are older than the Tertiary beds with which they are associated. The volcanic rocks are cut by dykes of very basic rock resembling limburgite. The little hill west of Wittelsberg, near the northern edge of the basalt area, and the flank of the hill near Kehrenberg, are composed largely of basalt tuff.

The basalt consists of phenocrysts of augite and olivine in a dense felt of augite microlites, biotite and magnetite, in the spaces between which is a colorless glass containing xenomorphic feldspar, leucite and nepheline. Inclusions in the basalt are very common. They comprise besides fragments of foreign rocks, concretions of olivine and of augite. The olivine concretions always contain more or less bronzite, and usually they are surrounded by a violet-brown rim similar to the rims found surrounding the augite phenocrysts in the basalt. Even those concretions that are composed almost exclusively of bronzite are surrounded by rims of this character. The principal component of this rim is a monoclinic augite, so that it appears here that the bronzite, which must have been one of the earliest separations from the magma, was, after its crystallization, changed into augite. Other concretions show the

[15] Am. Jour. Sci., L, pp. 480–488, Dec., 1895.

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

alteration of the bronzite into olivine. By complete fusion one concretion, which is thought by the author to have been a bronzite-augite aggregate, has been changed to a mass of rounded augite and olivine grains imbedded in a glass which locally is replaced by nepheline. The alteration of the bronzite, as indicated by the study of a number of sections, is into olivine, augite, magnetite and glass. Among the rare constituents of the olivine concretions are chrome diopside and picotite. The augite concretions or inclusions, consist almost exclusively of a monoclinic augite with which is usually associated a little olivine. In the interiors of the concretions the augite contains fluid enclosures, but toward their peripheries the enclosures are all of glass. Often between the augite grains are little nests of calcite. One of the inclusions observed by the author is abnormal in that it is composed of a small nucleus of augite surrounded by a zone of brown biotite.

Of the foreign inclusions, the author describes two kinds—the calcareous and the granitic. The basalt in the neighborhood of limestone inclusions loses its biotite and magnetite. Nearer the inclusions the augite microlites become light colored and magnetite grains are again developed. At the boundary of the limestone fragment is a rim of large augites, whose ends are directed toward the center of the inclusion. This latter itself is composed of the remnants of calcite grains imbedded in a brown glass, in which are also well formed crystals of a scapolite. The sandstone inclusions have been changed to a mass of quartz grains lying in a brown glass, the whole being surrounded by the usual zone of augite microlites. The granite inclusions first lose their mica. The old feldspar has given rise to newly developed feldspar.

The dolerite seem to occur as a number of small flows that have run together. It presents no special peculiarities. The dyke basalt cutting the tuffs and dolerites sometimes contains well defined crystals of olivine, which occasionally occur as interpenetration twins.

Igneous Rocks of British Columbia.—The petrographical characters of the principal rocks occurring within the area of the Kamloops Map-sheet of British Columbia are described by Ferrier.[2] These rocks embrace feldspathic actinolite schists, diabase porphyrites, harzburgite, amphibolites, diabase tuffs, cherts, gabbros, orthophyres, augite-porphyrites, porphyrites, basalts, pecrite-porphyrites, andesites, trachytes, dacites, diorites, granites, syenites, quartz-porphyries, alnoite and a series of much altered rocks. The descriptions are all brief.

[2] Annual Rep. Geol. Surv. of Canada, Vol. VII, Pt. B., p. 349,

Chalcedony Concretions in Obsidians from Colorado.—Patton[3] describes the occurrence of large opal and chalcedony concretions or geode-like bodies in beds of a decomposed obsidian on Ute Creek in Hinsdale Co., Colorado. The concretions are most common in the upper scoriaceous portions of the flows. Similar concretions were also found in a rhyolite at Specimen Mountain. The concretions are composed of radial fibres of chalcedony. The flowage lines that are common to the rock pass uninterruptedly through them, and in them are trichites exactly like those in the body of the rock. The concretions are regarded as secondary in origin—and as due to the percolation of silica-bearing waters through the rock. The same author publishes some photographs of erosion forms produced by the weathering of the volcanic conglomerates in the San Juan Mountains.

Basic Dykes near Lake Memphremagog.—According to Marsters[4] the Chazy limestones of Lake Memphremagog are cut by granite, olivine, diabase and lamprophyre dykes. The latter comprise dark rocks containing phenocrysts of augite, hornblende or olivine. The olivine, when it occurs, is always situated in the central portions of the dykes. Sometimes its crystals are one and half inches in diameter. Petrographically these rocks are augite camptonites, fourchites and monchiquites. The augite camptonite contains both augite and hornblende in two generations and in varying quantities. Only two fourchite dykes were observed. Their material presents no unusual features. The paper is interesting as bringing to our knowledge another area in which these peculiar and interesting dyke rocks occur.

The Origin of the Maryland Granites.—The last article written by the late Dr. Williams[5] is an introduction to Keyes article on Maryland granites. In this paper the author explains the criteria by which ancient plutonic rocks may be recognized in highly metamorphosed terranes, and applies the principles thus established to prove the eruptive nature of many of the Maryland granites. The pegmatites of the Piedmont plateau were tested by the same criteria, with the result that these too are pronounced to be eruptive. Many handsome plates embellish this portion of the paper. In the main portion of the article Keyes describes the petrographical features of the different types of granite, giving special attention to the original allanite and epidote found in them. There is little that is new in the paper, most of its

[3] Proc. Colo. Scient. Soc , Nov. 4, 1895.
[4] Amer. Geol., July, 1895, p. 25.
[5] 15th Ann. Rep. U. S. G. S., 1895, p. 653.

essential points having already been discussed by Hobbs, Grimsley and others.

Petrographical Notes.—The rocks of the Laurentian area to the north and west of St. Jerome, Quebec, are briefly referred to by Adams[6] as gneisses, anorthosites, amphibolites, limestones, quartzites, etc. Some of the gneisses are eruptive and others are probably sedimentary.

Miller and Brock[7] have found in Frontenac, Leeds and Lanark Counties, Ontario, granites, gabbros, scapolite and pyroxene rocks of Laurentian age cut by dykes of quartz gabbro containing phenocrysts of pyroxene and plagioclase.

Keyes[8] declares that the granites and porphyries occuring in the eastern portion of the Ozarks, in Missouri, "are very closely related genetically, and are to be regarded as facies of the same magma," the porphyry being the upper and surface facies of the granite.

GEOLOGY AND PALEONTOLOGY.

Canadian Paleontology.—In addition to the vertebrates (reptilia and batrachia) and land snails discovered by Sir Wm. Dawson in the interior of erect trees in the coal formations of Nova Scotia, and described by him in various scientific publications, fragments of arthropods have been found in the material collected. These were submitted for examination to Mr. Samuel Scudder who published a preliminary report in 1882, and now, after completing his study, gives these additional facts. A few species of Myriapods show traces of the bases of spines; the ventral plates in Archiulus are very broad; two new species of this genus are recognized; two species of Mazonia are indicated, one of which (*M. acadica*) confirms the separation of this genus from Eoscorpius; a facetted eye taken from a reptilian coprolite shows the presence of a true insect, probably a cockroach.

A report upon the Cenozoic Hemiptera of British Columbia, by the same author, comprises descriptions of nineteen species. Mr. Scudder calls attention to the great variety among these insects. Among the Homoptera, every specimen must be referred to a distinct species, and

[6] Ann. Rep. Geol. Surv. of Can., Vol. VII, J., p. 93.
[7] Can. Record of Science, Oct., 1895.
[8] Bull. Geol. Soc. Amer., Vol. 7, p. 363.

in only one case can two species be referred to one genus. In the Fulgoridae each of the three species belongs to a different subfamily. Another striking feature of the fauna is the size of the individuals which compose it. The majority of them represent the most bulky species of their respective families. The average length of these Cenozoic species of Fulgoridae and Cercopidae is not less than two centimeters, and there are some that are double that length.

The author states that this insect fauna indicates that the deposits in which they occur are at least as old as Oligocene, but no definite statement as to the age of the beds can be made.

A third interesting paper in this series on Canadian fossil insects sums up the present knowledge of the Coleopterous remains of Canada. These have been found in seven distinct localities in that country, and at three very different horizons. The greatest interest attaches to the collection made at an interglacial locality near Scarboro' Ont., which yielded twenty-nine species, and is the largest assemblage of insects ever found in such a deposit anywhere. Forty-five species from the various localities are described by Mr. Scudder. They are referred to 27 genera, 2 of which are new. (Contrib. Canadian Paleontol., Vol. II, Pt. I.)

Jackson on the Development of Oligoporus.—The following is an abstract of the results of the recent studies of the Palæoechinoidea. In Oligoporus the interambulacra terminate ventrally in two plates, which present on their oral faces a reëntrant angle for the reception of a single initial plate of the area. Proceeding dorsally, new plates and new columns of plates are added, accenting by their appearance stages in growth, as he had previously shown in Melonites, until the full compliment of the species is attained. The single initial interambulacral plate of Oligoporus was compared with a similar plate in Melonites, Lepidechinus, young modern Cidaris, etc. At the ventral or younger portion of the corona of Oligoporus there are only two columns of ambulacral plates. The four columns characteristic of the adult are derived from these two by a drawing-out process. The four columns of ambulacral plates of adult Oligoporus are the equivalent of the two outer and two median columns of Melonites. These four columns in both genera are the morphological equivalent of the two columns seen in the ambulacra of Bothriocidaris, Cidaris, etc.

Oligoporus, as shown by the development of both ambulacral and interambulacral areas, is a genus intermediate between Palæechinus and Melonites. During the development of Oligoporus it passes

through a Rhoëchinus stage, and later a Palæechinus stage. Melonites in its development passes through an Oligoporus stage.

An early stage in developing Echinoderms was named the "protechinus" stage. At this stage are first acquired those features which characterize the developing animal as a member of the Echinoidea. The protechinus stage in Echinoderms is directly comparable to the protoconch of Cephalous Mollusca, the protegulum of Brachiopods, the protaspis of Trilobites, etc. The Echinoderm at this period in its growth has a single interambulacral plate (representing a single column of such plates), and two columns of ambulacral plates in each of the five areas. This stage is seen in Oligoporus, Lepidechinus, Goniocidaris and other genera; it finds its representative in an adult ancestral form, in the primitive, oldest known genus of the class Bothriocidar is of the Lower Silurian, which has but one column of interambulacral and two columns of ambulacral plates in each area.

Species of Oligoporus and Melonites with few interambulacral columns are considered the more primitive types, as they are represented by stages in the development of those species which acquire a higher number of columns in the adult.

The structure of the ventral border of the corona of Archæocidaris was described. It presents a row of plates partially resorbed by the encroachment of the peristome, as in modern Cidaris, etc. Ambulacral and interambulacral plates on the peristome were described in Archæocidaris, also teeth and secondary spines on the interambulacral plates of the corona.

This paper contains a classification of Palæozoic Echini based on the structure and development of the ambulacral and interambulacral areas and the peristome. It will be published in the Bulletin of the Geological Society of America.—Science, Nov. 22, 1895.

American Fossil Cockroaches.[1]—This memoir, published as Bulletin 124 of the U. S. Geological Survey, is a revision of the known species of American fossil cockroaches to date. The descriptions of new forms are interpolated in a systematic list of all the species yet recovered from the rocks, and such tables have been added as may enable the student to readily determine any new material. With the publication of this essay all species hitherto described will have been figured.

[1] Bulletin of the United States Geological Survey, No. 124. Revision of the American Fossil Cockroaches, with Descriptions of New Forms. By Samuel H. Scudder, Washingtou, 1895.

The new forms are Paleozoic, and are mostly from two new localities—Richmond, Ohio, and Cassville, West Virginia. There are, however, a number of new species from old horizons.

Tables of the geographical and also of the geological distribution of both American and European genera are given in the introduction, followed by a statement of the characteristics of the Mylacridae and a discussion of some of the anatomical features of paleozoic cockroaches. In this connection the author calls attention to possible mimicry among these old forms of insect life, and figures side by side a cockroach wing and a fern frond found associated in the same beds, to show how close is the resemblance between them in the general distribution of nervures and in outline.

The illustrations comprise twelve page plates and three figures in the text.

The Comanche Cretaceous.—Prof. R. T. Hill has found some outlying areas of the Comanche series in Barber and Comanche Counties, Kansas, and in G County, Oklahoma, and in the Tucumcari region of New Mexico. These strata are identified from paleontological evidence.

The importance of a correct determination of these beds is evident from the following concluding remarks of the author.

"The geology of the outlying areas of the Cretaceous preserved in the scarps of the Plains adds greatly to our knowledge of the distribution, variation, paleontology and history of the beds of the Comanche series, and of the progressive oscillatory conquest of the Great Plains region by the sea in Cretaceous time. The Belvidere (Kansas) beds have revealed the following additions to our knowledge of Cretaceous paleontology: First, a lower stratigraphic occurrence of the dicotyledonous Dakota flora than known, whereby we may now say that dicotyledons make their first appearance before the beginning of the Washita subepoch, instead of in the Dakota as hitherto believed. Second, a similar downward range in the geologic scale of the ichthyic vertebrates of hitherto supposed Upper Cretaceous range. Third, intermingling of these plants and fishes with molluscan species and other vertebrates of the Washita division such as has not hitherto been found in the Comanche series." (Amer. Journ. Sci., Lol. L, 1895).

Kolguev Island, which lies 130 miles southeast of Novaya Zemlya, differs, according to Col. Feilden, in geological structure, both from mountainous islands of its neighbor and from Russian Lapland. The entire elevated region of the island is composed of beds of sand contain-

ing erratic boulders. to a depth of not less than 80 feet, and these sandy beds rest on the Kolguev clays. These in turn are 50 miles long by 40 wide, with a thickness of not less than 250 feet, probably more. This great mass is evidently a glacio-marine deposit. A few molluscan remains were found in it, all well known boreal forms existing at the present time, but no vertebrates nor drift-wood. A collection of erratics made by the author are identified by Prof. Bonney as rocks of Mesozoic age, either Jurassic or Wealden. (Quart. Journ. Geol. Soc., 1896.)

Palæontologia Argentina.—Vols. I (1891), II (1893), and III (1894).—The Museo de la Plata of Argentina has progressed thus far with the publication of monographs illustrative of its magnificent collection of fossil vertebrata of that country. The style of the publication is worthy of the subject; the size selected being folio, and the plates phototype reproductions of the originals, often of the natural size. The whole is issued under the supervision of the director of the Museum Dr. Francisco P. Moreno, who contributes some of the articles in connection with M. Mercerat; while Dr. Lydekker, of London, furnishes the greater number.

The first volume, on the extinct birds of Argentina, consists solely of plates, with pages of names referring to the figures. These plates depict objects of great interest, many of the bones belonging to the extraordinary family of the Phororhacidæ of Ameghino, which seem to be nearly allied to the existing Cariamidæ of South America. Most of these birds are of gigantic size, and their powerful legs and hooked beaks indicate that they were quite competent to maintain their place in the fauna of which they form a part. We have waited for some years before noticing this valuable publication, in hopes that the text would appear. It seems, however, that there is no intention of publishing a descriptive part. Under the circumstances we must regret that names were attached to the figures, for, although figures may give currency to specific names, they cannot do so for names of any higher grade, and a considerable amount of synonymy has been thus created. Dr. Ameghino has also subsequently shown, that in this atlas a good many duplicate names have been given to the same species.

In the second part are published three memoirs by Dr. Lydekker. These include figures and descriptions of Dinosauria and Cetacea from Patagonia, and mammalia Ungulata from the same region. The magnificent plates are accompanied by descriptions, and this volume is therefore more valuable than its predecessor. Unfortunately the de-

scriptions are quite inadequate, and the specimens will have to be more fully described before their characters can be sufficiently known.

The third volume is chiefly occupied with the Edentata, and this memoir is admirably illustrated. The descriptions (by Dr. Lydekker) are rather more full than those of Vol. II, but not full enough. They are marred by frequent supercilious references to Dr. Florentino Ameghino, who is the most competent paleontologist of the vertebrata in South America, and whose descriptions compare very favorably with those of other paleontologists in all respects. His figures are not so good as those of the work now under review, for here we have a case in which the most skilful hand has not had the financial advantages it ought to have had. From our past experience we should say that when Dr. Lydekker states that organic forms are distinct species he is apt to be correct; but when he identifies forms alleged to be distinct, further examination is in order.—C.

BOTANY.[1]

Tilden's American Algæ.—The first century of this distribution by Josephine Tilden, of Minneapolis, was sent out about a year ago, but has not hitherto been noticed in these pages. The specimens are very neatly prepared, and are attached to cards or mica slips. In most cases they contain an abundance of material, but, in a few instances, we might wish for more generous specimens. The species represent the following genera:

Oedogonium (4), *Sphaeroplea* (1), *Hormiscia* (2), *Chaetophora* (4), *Draparnaudia* (3), *Stigeoclonium* (6), *Conferva* (1), *Microspora* (1), *Urospora* (1), *Cladophora* (15), *Pithophora* (1), *Vaucheria* (5), *Botrydium* (1), *Hydrodictyon* (1), *Tetraspora* (2), *Palmella* (1), *Protococcus* (3), *Euglena* (1), *Spirogyra* (10), *Cosmarium* (1), *Porphyrosiphon* (1), *Symploca* (2), *Lyngbya* (2), *Phormidium* (1), *Oscillatoria* (8), *Spirulina* (1), *Gloeotrichia* (2), *Tolypothrix* (1), *Nostoc* (3), *Anabaena* (2), *Merismopedia* (1), *Navicula* (1), *Pleurosigma* (1), *Gomphonema* (2), *Cocconeis* (1), *Nitzschia* (1), *Odontidium* (1), *Synedra* (2), *Fragilaria* (1), *Cystopleura* (1), *Lysigonium* (1).

The introduction of *Euglena* among plants is, in our opinion, a mistake, although one which will probably do no harm, since it will be difficult if not impossible to recognize them from dried specimens.

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

Century II is announced to appear soon. We bespeak for it a liberal patronage.—CHARLES E. BESSEY.

The Columbines of North America.—Thirteen species of *Aquilegia* are described as occurring in North America in Robinson's edition of Gray's Synoptical Flora (1895).

These fall into two types, as follows:

A. Old World type, with hooked or curved spurs:

A. brevistyla, Rocky Mountains of British America, and the Black Hills of South Dakota.

A. saximontana, Rocky Mountains of Colorado.

A. flavescens, Pembina and British Columbia to Oregon and Utah.

A. micrantha, southeast Utah.

A. ecalcatata, southwest Colorado.

A. jonesii, northwest Wyoming and Montana.

B. American type, with straight spurs:

A. canadensis, common east of the Rocky Mountains.

A. formosa, Alaska to northern California, Idaho and Utah.

A. truncata, California.

A. caerulea, Rocky Mountains from Montana to New Mexico.

A. chrysantha, southern Colorado to New Mexico and Arizona.

A. pubescens, California.

A. longissima southwest Texas.

It is interesting to note that in Torrey and Gray's Flora of North America (1840) there were but four species described, viz.: *A. canadensis*, *A. formosa*, *A. caerulea* and *A. brevistyla*. It is possible that some of these species may be reduced to varieties upon a more critical study of the genus, but even with the most rigid reduction we should still be left with a large representation of these interesting plants. Their curious beauty and comeliness, with their general distribution, may well warrant the suggestion which has been made to make the Columbine our national flower.—CHARLES E. BESSEY.

Sets of North American Plants.—Two sets of peculiarly interesting North American flowering plants attract the attention of herbarium curators at this time. They consist very largely of species from Florida, that wonderfully rich semi-tropical region whose botanical treasures we are just learning to appreciate. The first is a set of 400 specimens by the veteran collector A. H. Curtiss, of Jacksonville, Florida. A personal examination of the specimens warrants the same high commendation which all of Mr. Curtiss's work has hitherto received.

The second set is published by G. V. Nash, of Washington, D. C., and includes the same number of specimens. A glance at the list shows it to include many rare and a considerable number of new species. Either set would be a valuable acquisition to any college herbarium.—CHARLES E. BESSEY.

Botany in Buffalo.—The Secretary of the Section of Botany (G) of the American Association for the Advancement of Science, Professor George F. Atkinson, of Ithaca, N. Y., is making an effort to provide a good programme for the meeting in August (24 to 28). Titles and abstracts of papers are to be sent to the Secretary not later than July 1, in order that they may be arranged and forwarded to the Permanent Secretary of the Association for printing and distribution. It is the purpose of the Association to issue such a list of Section programmes not less than a month preceding the meeting. Let every botanist who has something of importance send in his title *and abstract* on or before the first day of July.

The second annual meeting of the Botanical Society of America which will be held on August 21 and 22, in connection with the Association, should attract a good number of the more advanced men in the science. Dr. Trelease, the retiring president, will deliver his address on "Botanical Opportunity" at 8 P.M. of the 21st. On the 22d there will be forenoon and afternoon sessions for the reading of papers and discussions.—CHARLES E. BESSEY.

Blanks for "Plant Analysis."—For some time there has been an encouraging decrease in the annual crop of blanks for "plant analysis," and we hoped to be able soon to announce the complete extinction of the species. It appears, however, that there are certain intellectual soils in which they still thrive, in spite of the fact that, like the Russian Thistle, they are outlawed in most communities. We have before us two which bear the date 1896, one from U. O. Cox, of Mankato, Minnesota, and the other from H. J. Harnly, of McPherson, Kansas. If one may distinguish between things which are necessarily bad, it may be said that the first is the better of the two. Its fault (which is fatal) is that it enables the pupil to "analyze" a plant with the least possible thinking: he does not have to remember anything; he merely reads the question, looks at his plant, and makes his entry on the proper line. The second blank (which is "copyrighted") adds to the foregoing much which is confusing and scientifically vicious. Thus the pupil finds the questions "Flowers, Regular or Irregular? Why?" which he is expected to answer in a line just *two and a half inches long!*

Again he is asked, "Flowers, Complete or Incomplete? Why?" and is allowed a line exactly two inches long in which to give an answer to a question before which the wisest botanist may well quail. When will teachers realize that botanists are not made by the use of such "helps" any more than Latin scholars are made by the use of "ponies"?

—CHARLES E. BESSEY.

Botanical News.—The Director of the Missouri Botanical Garden at St. Louis calls attention in a printed circular to the advantages for study afforded by this important institution. Its herbarium includes nearly 250,000 specimens, and its library about 10,000 volumes and 11,000 pamphlets.

A. H. Curtiss, of Jacksonville, Florida, is distributing fine sets of the Marine Algæ of Florida. Each set contains fifty species and is sold for five dollars.

Professor Bruce Fink, of Fayette, Iowa, offers sets of Iowa Lichens, including about 200 species which he sells at the low price of six cents each.

We are glad to see another number of *Pittonia*, the very useful periodical which Professor E. L. Greene issues from time to time. The new part (13) contains papers on the Nomenclature of the Fuller's Teasel, a Proposed New Genus of Cruciferae; New or Noteworthy Species; New Genus of Polemonianae, and New Mexican Eupatoriaceæ—CHARLES E. BESSEY.

ZOOLOGY.

Japanese Leeches.—The discovery of three new land leeches in Japan is of interest to geologists since but one species, *Haemadipsa japonica* Whitman, is all that has been known to occur in that country. The three new species are members of a genus separated from all the genera of land leeches hitherto defined. An account of their external characters and a general outline of their internal organization are presented by Dr. Asajiro Oka in a recent number of the journal published by the Imperial University of Japan. For the new genus the author proposes the name Orobdella. The species of this genus are found in various mountainous parts of Japan, crawling under moss and fallen leaves, or in moist earth, in the same manner as earthworms, which con-

stitute the chief source of their nourishment. Having no jaws, these leeches can neither bite nor suck blood, but swallow the worms entire. *O. octonaria* is one of the largest leeches known. The dimensions of one specimen found by the author is given, length 270 mm., width 14 mm., depth 10 mm.

Dr. Oka adopts the classification of R. Blanchard (1894), and shows the systematic position of Orobdella in the following synoptical table:

Ordo Hirudinea.
- a. Subordo. Rhyncobdellae.
- b. Subordo. Arhyncobdellae.
  - 1. Fam. Gnathobdellidae.
    - Aquatic: gen. Hirudo, Haemopis, etc.
    - Terrestr. gen. Haemadipsa, Xerobdella, Mesobdella.
  - 2. Fam. Herpobdellidae.
    - Aquatic: gen. Herpobdella, Dina, Trocheta.
    - Terrestr: gen. Cylicobdella, Lumbricobdella, Orobdella.

(Journ. Coll. Sci. Imp. Univ. Japan., Vol. VIII, Pt. 2, 1895.).

The Origin of Tail-forms.—The use and meaning of the asymmetrical types of tail-fin which are so commonly met with among fishes—e. g., the upturned tail of the shark and sturgeon, and the downwardly extended fin of the flying-fish, are explained by Dr. F. Ahlborn by comparisons founded on experience in rowing. Every tyro knows the consequences which ensue if he holds his blade too obliquely in the water. If the upper edge is inclined too much towards the stern of the boat a brisk pull upon the handle results in the blade jumping out of the water; if, on the other hand, the blade is inclined too much in the opposite direction, it digs into the water and the oarsman "catches a crab." The relevance of these illustrations is found in the fact that the skeletal support of the asymmetrical tails of fishes is generally such that either the upper or lower border of the fin is more resistant to the pressure of the water than the opposite border, a fact which causes the fin in action to assume an oblique instead of a vertical position. The result of such a disposition is that in those cases where the upper part of the tail is stiffer than the lower, the tail in locomotion is driven upwards, as the oar is driven out of the water (heterocercal tail of shark and sturgeon); while in cases where the lower part of the tail is firmer than the upper, the tail tends, in action, to assume a lower position than the rest of the body (flying-fish). The body of the animal, in fact, is made to swing vertically about a horizontal axis running through the center of gravity: in the first group

the tail becomes elevated above the head, in the second group the head becomes raised above the tail. The utility of these types of organization becomes obvious when the habits of the creatures which exhibit them are considered. The first group consists of bottom-haunting fish, which are thus enabled to give free play to their tails while scouring the sea-bottom in search of food; the second consists entirely of surface-swimming forms which are enabled, by this beautiful adaptation of structure, to swim swiftly beneath the surface of the water without the risk of their tails emerging, and so cause inconvenience and waste of force. The tails of many air breathing aquatic animals, such as the sea-snake and the extinct Ichthyosaurus are constructed upon this latter principle. (Nature, Feb., 1896.)

The Spermatheca in some American Newts and Salamanders.—The term *receptaculum seminis* has been used to designate certain structures in the cloacal wall of the female *Necturus maculatus*, which serve as reservoirs in which the zoösperms of the male are received. In order to have a better understanding of the function of these structures, Dr. Kingsbury undertook a study of the cloaca in the female of six species of Urodeles (American). The chosen species represent five families, and two orders of Batrachia, and present a good series from a purely aquatic to as purely a terrestrial existence. The general result has been a recognition of these organs in one form or another in all the species under observation, but there is no unity of structure, hence the term *receptalum seminis* is not strictly applicable, and the mononym *spermatheca* is proposed instead. In some forms many spermathecas would be recognized.

In Diemyctylus, Amblystoma and Necturus the spermathecas assume the form of individual tubules. In Amblystoma the tubules are arranged around depressions. In Spelerpes, Plethodon and Desmognathus consists of a tubular depression of the cloaca into the end of which the clustered tubules open.

As to how the spermatozoa find their way into these resting places, the author suggests that while the theory of Pfeffer of "positive chemotaxis" is highly probable, yet it is also possible that the entrance of the zoösperms may be solely due to their own activity assisted by muscular contractions of the cloaca and spermatheca.

The results of Dr. Kingsbury's observations are thus summarized:—

1. In the genera Necturus, Amblystoma, Diemyctylus, Plethodon and Desmognathus, spermathecas are found in the dorsal wall of the

cloaca of the female, containing zoösperms. Internal fertilization is therefore proven for these forms.

A spermatheca occurs in Spelerpes; in the single specimen examined (taken in the fall) no zoösperms were contained.

In Necturus, Diemyctylus and Amblystoma, there are several tubules or spermathecas opening upon the cloacal epithelium, which serve as reservoirs for the semen.

In Desmognathus, Plethodon and Spelerpes, there is a single mesal spermatheca.

The condition in Spelerpes would seem to indicate that the organ in these latter genera equals the group of tubules found in the first genera plus and exaggerated and modified depression of the cloacal epithelium, such as occurs in Amblystoma.

2. No gland-like structures in addition to the spermatheca occur in the female of Plethodon and Desmognathus.

3. In all the remaining genera a ventral cloacal gland is present.

4. In Amblystoma, Spelerpes and Necturus, in addition to the spermatheca tubules, other tubules occur on the dorsal side of the cloaca.

5. The secretion of the cloacal glands is employed at the time of ovulation.

6. The three glands of the male recognized in the Triton, the cloacal, abdominal and pelvic, occur and are well developed in the five genera examined. This suggests that by all of these spermatophores are deposited.

7. A résumé of the literature and foregoing facts points to a uniform mode of mating and fertilization in all urodeles.

8. Dorsal and ventral ciliated tracts occur in the male of all the genera examined. Cilia in the cloaca of the female were detected only in Amblystoma and *Plethodon glutinosus*, where the tract was not as extensive as in the male. (Proceeds. Amer. Microscop. Soc., Vol. XVII, 1895.)

Zoological News.—A second species has been added to the genus Opisthoteuthis founded by Verrill to receive a West Indian species named *O. agassizi*. The new acquisition was obtained by a Misaki fisherman with a hook at a depth of about 25 fathoms in Iagami Bay, Japan. It is described and figured by Dr. Ijima and S. Ikeda under the name *O. depressa*. (Journ. Coll. Sci. Imp. Univ. of Japan, Vol. VIII, Pt. 2, Tokyo, 1895.) This genus is characterized by the fact that the alimentary canal passes directly through the body, instead of

returning to issue near the mouth. Ferrill regards it as the most primitive form of the Cephalopoda.

A new genus of Cottoid fishes from Puget Sound is described by Mr. E. C. Starks. The type species, *Jordania zonope* is in the Museum of the Leland Stanford, Jr., University. (Proceeds. Phila. Acad. Nat. Sci. [1895] 1896).

Mr. J. A. Allen emphasizes the fact that the change of color in the plumage of birds without moulting is due to the gradual wearing off of the light colored edges of the feathers, combined with the more or less blanching of the color of certain parts. Exposure to the elements and friction also produce more or less marked change in color. The author prefaces his remarks with a brief history of origin and persistence of the theory unwarranted by the facts that the feathers of birds change color with the season independent of the process of moulting. (Bull. Amer. Mus. Nat. Hist., Vol. VIII, 1896.)

ENTOMOLOGY.[1]

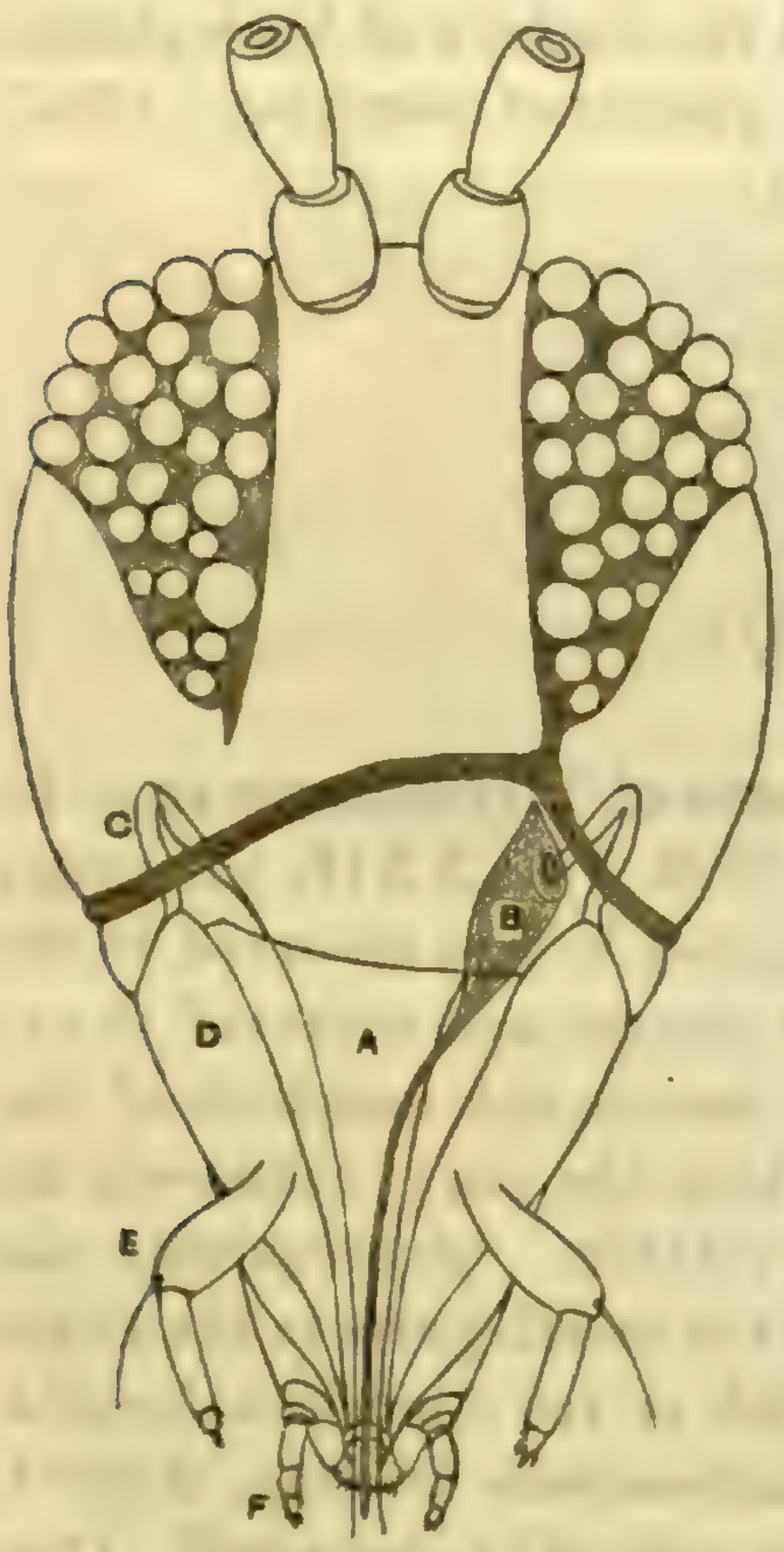
The Asymmetry of the Mouth-parts of Thysanoptera.—In the Bulletin of the Essex Institute, for 1890, Vol. XXII, the writer published a brief account of some peculiarities he had observed in the mouth-parts of members of this order of insects, and ventured in explanation, the hypothesis that in these insects the mandible of the right side of the head is wanting, and that the parts commonly regarded as mandibles are lobes of the maxillæ. Subsequently the writer called this anomalous condition of the mouth-parts to the attention of members of the Entomological Club of the American Association for the Advancement of Science (Indianapolis meeting, August, 1890) and presented slides showing the peculiarities described. (See Canadian Entomologist, 1890, Vol. XXII, p. 215.)

Nothing, so far as I know, has appeared in American literature since that time with reference to the matter, and the old view concerning the structure of the mouth seems to be still current. In Prof. J. H. Comstock's excellent manual, recently issued (1895) the labrum is represented as perfectly symmetrical, the parts considered by him to be mandibles are incompletely represented, and no mention is made of

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

any unusual feature of the mouth structure. In giving the characters of the order, he says: "The mouth-parts are probably used chiefly for sucking; they are intermediate in form between those of the sucking and those of the biting insects; the mandibles are bristle-like; the maxillæ are triangular, flat, and furnished with palpi; the labial palpi are also present."

I have just examined a copy of Jindrich Uzel's "Monographie rádu Thysanoptera," 1895, perhaps the most extensive work yet published on the Thysanoptera, in which the view of the mouth structure described by me in the Bulletin of the Essex Institute and before the Entomological Club is adopted, though Uzel is disposed to take a different view of the homology of the unpaired mouth-part. His words are (Ibid, p. 25): "In der Höhlung des Mundkegels bewegen sich die Mondibeln in Form zweier Stechborsten und der unpaare Mundstachel (wohl ein umgebildeter epipharynx) welcher linkerseits liegt und den für die Thysanopteren characterischen Unsymmetrischen Bau der Mundwerkzenge bedingt." In order to show more clearly what the unpaired part is like, I have made a drawing from his Figure 161, Tab. IX, which is here reproduced.



Of the interpretation Uzel is disposed to put upon the unpaired part, I have only this to say: It is plainly closely adapted to the left side of the head, and the parts belonging to the region in which it lies are closely adapted to it. It is very evident that it was made to fit in the angle formed by the hard parts of the head on the left side; the labrum is, on this side, shortened and otherwise suited to accommodate it. A reëxamination of my slides shows the adjustment more complete even than represented in Uzel's figure. Coupled with this is a manifest deficiency in the head on the right side at the place where a corresponding structure should be. It is evident that something is lacking on the right side. If the unpaired organ is an epipharynx that has been displaced, why should the cranial structure of the right side be altered?

The further question arises, why should an epipharynx be pushed to one side and completely shaped to the structures there?

I have suggested that the pair of slender parts, called by Uzel and others mandibles, may be lobes of the maxillæ, and urged in explanation that they are attached to parts regarded by everybody as maxillæ, and besides that they are composed of two divisions (Professor Comstock does not represent the basal piece at all and hence the slender distal part appears in his figures as if free from the maxilla). Uzel figures this pair of mouth organs, as I have done, attached to the bases of the palpus-bearing parts, and as composed of a short basal piece and a long slender distal one. He says nothing of their jointed character but represents an articulation in the right one of his figure 161. They appear to me to be two-jointed and with this true, to consider them mandibles is to assume a departure from the one-jointed condition of the mandible prevailing in Hexapoda.

In the proceedings of the Entomological Club of the American Association (Can. Ent., Vol. XXII, p. 216), I am reported as stating that two unmistakable tarsal claws are present in Plæothrips and that the vesicle is probably a modified pulvillus. Prof. Comstock says: "The tarsi are two-jointed, bladder-like at the tip, and without claws." Uzel, on the contrary, states that claws are more or less developed in all Thysanoptera. "Beine Kurz; der eine-bis zweigliedrige Tarsus am Ende mit zwei mehr oder wenlger deutlichen Klanen, welche an Blase anwachsen."

Prof. Comstock's work has been quoted simply because it represents the established American view of the structure of Thysanoptera, a view which must certainly be changed in some particulars. To what extent the asymmetry referred to has been studied by foreign entomologists I am unable to say, since I have not been so situated as to be able to keep close track of the foreign literature. Thus far I have seen no reference to it except that in Uzel's work.

Explanation of the Figure.

Front view of head of *Aeolothrips fasciata*. A, the unsymmetrical labrum; B, the unpaired mouth-part (mandible, according to my interpretion, epipharynx according to Uzel); C, lobe of maxilla (mandible of Uzel and other authors); D, the maxilla; E, the maxillary palpus; F, the labial palpus.—H. Garman.

A New African Diplopod Related to Polyxenus.—While collecting insects in the darker parts of the Liberian forests, I have on

a few occasions noticed what appeared to be large individuals of Polyxenus of a dark brownish color, running about on the smooth leaves of the shrubby undergrowth, several feet from the ground. To preserve and carry to America specimens in satisfactory condition is not easy, and hence the present purpose of describing the external features of one found yesterday and having nearly all the bristles still in place.

SAROXENUS g. n.

Body minute, tapering caudad.

Head rounded, not as broad as the first segment; between the eyes with an anterior crescentic tiara of long upright serrate bristles: on each side between and above the eyes a short curved line of similar hairs elsewhere the head is smooth.

Eyes of a few (six?) small ocelli clustered on lateral prominences of the head.

Antennæ long and slender, distinctly clavate; sixth joint longest and much the thickest; seventh slightly longer than any of the proximal; eighth joint distinct, minute, several times smaller than the seventh.

First segment with six tufts of bristles, two in front, two behind and one on each side; the dorsal tufts are broader transversely; the lateral are raised on large projections, as in Polyxenus, and include more numerous and longer bristles.

The following six segments have each four tufts of similar bristles, two lateral and two posterior, the latter broad, as on the first segment; the bristles are longer and the tufts larger on posterior segments.

Last segment with a nearly complete transverse row of divergent bristles just in front of the dense brush of much finer, closely compacted bristles which compose the terminal fascicle.

Saroxenus scandens sp. n.

General color dark grayish brown, the terminal fascicle nearly white; in alcohol and under the microscope, the bristles of the head and segments are seen to be dark brown; the distal joints of the antennæ and legs are pinkish brown, and the exposed portions of the integument have a tinge of the same color; integument generally waxy or dirty white, and transparent so that the contents of the alimentary canal are visible as a dark line; eye spots dark brown.

Segments 8, though the specimen may not be mature; ten pairs of legs.

Length 3.5 mm., or with the terminal fascicle 4 mm.; width 1.2 mm., including the bristles.

Locality, Running about on the leaves of undergrowth, in the forest on Cape Mesurado, Liberia.

Under sufficient magnification the bristles of the head and segments appear as round hollow structures with about four longitudinal rows of very fine appressed teeth directed distad. The bristles of the terminal fascicle are more slender and have for a part of their length large appressed spines in opposite pairs something as shown by Latzel for *Polyxenus lagurus*. Nothing was seen similar to the apices of the hairs as figured by the same author.

This new genus is to be distinguished from Polyxenus and Lophoproctus[2] by the form of the antennæ and the distribution of the dorsal setæ. In Polyxenus the antennæ are short; in Lophoproctus they are long, but the apical joint is subequal to the penultimate.

Polyxenus has two transverse dorsal rows of rather remote short clavate and strongly serrate setæ, while Lophoproctus has a single row. The type of the latter genus is eyeless, although Mr. Pocock proposes to iuclnde a species with eyes, *Polyxenus lucidus* Chalande.

From the West Indes Mr. Pocock has described another Polyxenus[3] which, to judge from the drawing, has four tufts of setæ on each segment, and also a scattering row along the posterior margin. The antennæ are said to be very long, but appear not to be clavate, and the relative proportions of the joints are not stated. It is probably the type of a new genus having affinities with the African rather than with the European forms.

By the discovery of Saroxenus the distribution of the Pselaphognatha is considerably extended. Should members of the group be found in other tropical regions there will be added assurance of the antiquity of the subclass, and of the probability of relationship with such fossils as Palæocampa.—O. F. Cook.

Monrovia, 1 Feb., 1896.

North American Crambidæ.—Dr. C. H. Fernald publishes as a bulletin from the Massachusetts Agricultural College an important Monograph of the Crambidæ of North America. The author has long been recognized as tne leading authority on the micro-lepidoptera. The new genera Eugrotea and Pseudoschœnobius are characterized as well as several new species. The bulletin is admirably illustrated by three plates in black and white and six plates in colors, beautifully printed. This will certainly prove one of the most satisfactory entomological publications ever issued from the Agricultural Colleges.

[2] Pocock, Ann. Mus. Civ. Genova, XXXIV, 506.

[3] *Polyxenus longisetis*, Journ. Linn. Soc., XXIV, 474.

New Mallophaga.—Much the most important paper as yet published in America concerning the Mallophaga is the recent contribution from the Hopkins Seaside Laboratory, in which Prof. V. L. Kellogg treats of New Mallophaga, with special reference to a collection made from Maritime birds of the Bay of Monterey, California. In the 140 pages of print the author presents descriptions and figures of one new genus and thirty-eight new species of Mallophaga, together with twenty-two species previously described by European authors, but now, with few exceptions, first determined as parasites of American birds. In addition, the paper contains an excellent general account of the Mallophaga and fourteen admirable plates. It can be obtained for 50 cents by addressing The Registrar, Stanford University, California.

Entomological Notes.—Professor D. S. Kellicott publishes[4] the second part of his excellent Catalogue of the Odonata of Ohio. It deals especially with the species of the southern part of the State.

In Bulletin 32 of the Iowa Experiment Station, Messers. Osborn and Mally treat of the chinch bug, four-spotted pea-weevil, the imbricated snout-beetle and other injurious species.

Bulletin 62 of the Virginia Station contains a discussion of the San Jose Scale, by Wm. B. Alwood.

In Bulletin No. 2 of the Technical Series from the U. S. Division of Entomology, Mr. L. O. Howard publishes a careful account of The Grass and Grain Joint-worm Flies and their Allies, being a consideration of some North American Phytophagic Eurytyminæ.

In the issue of the Entomologist's Record for May 1st Mr. J. W. Tutt begins an interesting series of articles upon Mimicry.

In Bulletin 69 of the Ohio Station, the Chinch Bug is discussed at length F. M. Webster.

Prof. S. W. Williston publishes a useful Bibliography of North American Dipterology, 1878–1895, in the January Kansas University Quarterly. In the same issue W. G. Snow gives a List of Asilidæ supplementary to Osten Sackens Catalogue of North American Diptera, 1878–1895.

[4] Jour. Cin. Soc. Nat. Hist., XVIII, 105–114.

EMBRYOLOGY.[1]

Protoplasmic Continuity.—Prof. Hammar, of Upsala,[2] emphasizes by figures and description the connection of the cells of the egg of a cleaving sea urchin known to Selenka and others, but hitherto regarded as of no importance. He finds a thin outer layer on the cells of the early and later cleavage cells and even on the cells of the blastula. This layer is seen both in living and in preserved and sectioned material. Its appearance is not that of a membrane but, the author thinks, rather that of an "ectoplasmic" outer part of the protoplasm of the cell. This outer layer is very thin and might be easily overlooked.

It extends continuously over the entire egg and as it seems to be a part of each cell, all the cells are thus held together by a continuous outer pellicle that the author thinks is a protoplasmic layer.

This actual connection of the cells at their outer surfaces, if really a protoplasmic connection, should, as the author insists, be of great importance in the interpretation of the results of experimentation upon echinoderm eggs. He suggests that it offers a suggestion towards the explanation of the interaction believed to exist between the cells of a cleaving egg. Moreover such a connection would make clear why very different results have been obtained after shaking eggs and separating the cells *more or less*.

Cell Studies in Annelid Eggs.—Prof. E. Korschelt, of Marburg,[3] has made a most detailed and thorough study of the maturation and fertilization of the eggs of the small polychætous annelid, *Ophryotrocha puerilis* with special reference to the number of chromosomes concerned in cell divisions at different phases of the life history.

Many of the interesting facts described cannot be here referred to, but only some of those that bear upon the question of the value of chromosomes as permanent individuals.

The number of chromosomes found in dividing cells in the adult is *four*, in certain ectodermal, entodermal and mesodermal structures. This same number is found in the cells of the ovary and of the testis, the ancestors of the eggs and sperms. The same number is found in

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] Archiv f. mik. Anat., Marz 2, 1896.

[3] Zeit. f. wiss. Zool. 60, Dec. 31, 1895, pps. 543--680, pls. 28--34.

the early stages of cleavage and, as a rule, in the later stages and in the blastula, but in the later stages of cleavage and in the blastula there are often cells that contain *eight*.

In the maturation of the egg four chromosomes come out of the net work of the resting nucleus and eventually four go into the first polar body and two into the second. This is brought about as follows: The four very long chromosome loops shorten and divide lengthwise into four cleft rods. When these come to the equatorial region of the first maturation spindle they have again closed together so as to form four simple rods. These separate in pairs and move towards the poles of the spindle without presenting any true mitotic division. The first maturation division is, therefore, a reducing division. Yet the first polar body receives four chromosomes, since the pair that approaches that pole divides, as if opening out where previously split, and thus four rods are formed. The same takes place at the inner pole and four are left for the second maturation spindle. In the second polar body two chromosomes enter by moving away from the other two left in the egg. As it cannot be determined whether the pair entering the second polar body are two halves of one of original ones or halves of two original ones it is not certain whether the second maturation division is a reducing or an equating division.

Though the chromosomes are usually short rods or elongated granules during the maturation division, there are many eggs in which they appear as long, bent or horse-shoe shaped rods.

Some exceptions to the above account must be emphasized as showing the inconstancy of number of chromosomes resulting from lack of synchrony between chromosomal divisions and other phenomena of the cell.

Thus in some cases the first polar body has but *two* chromosomes, since the preceding division of chromosomes is left out. In others *eight* chromosomes are found at the equator of the first polar spindle, formed by a precocious division of the four chromosomes!

Fertilization takes place normally just after the eggs are laid and the sperm enters, while the first maturation spindle is still patent. In abnormal cases fertilization may take place inside the parent which is hermaphrodite and may ripen sperms and eggs simultaneously. Such cases, however, lead to abnormal cleavages and even to fusion of separate eggs, and seem due to some pathological state of the egg.

When the sperm enters the egg radiations are formed behind it, and later in front of it, so that the middle piece of the sperm may be re-

garded as introducing the centrosome or the archoplasm, and it is probable that the sperm revolves through 180°.

The male and the female pronuclei both move toward the centre of the egg and combine, but not till they have both gone through complex and similar changes, including the appearance and dissolution of an enormous nucleolus. The two nuclei finally fuse when each contains *two* long, thread-like chromosomes.

The centrosome or archoplasm of the maturation spindle disappears and that of the sperm divides and furnishes the first cleavage spindle. At the equator of this spindle are found the four chromosomes, two of male and two of female origin. Each splits lengthwise and the eight separate, so that each daughter nucleus obtains two chromosomes of male and two of female origin.

For many important facts not mentioned here the reader is referred to the two hundred remarkably clear figures and the judicial statements found in the original.

PSYCHOLOGY.

A Study in Morbid Psychology, with some reflections.—(*Continued from page 518*). With regard to the religious (?) experiences of Ansel Bourne, I am not so shallow as to think we can determine in the case of hallucinatory voices whether or no the phenomena are entirely subjective. An attitude of what the late G. J. Romanes has called "pure agnosticism" seems the only philosophical one in these difficult cases.[1] There has arisen a dogmatism in science as narrow and as mischievous as that of the straitest sect amongst theologians.

[1] "No one is entitled to deny the possibility of what may be termed an organ of spiritual discernment. In fact to do so would be to vacate the position of pure agnositicism *in toto*, and this even if there were no objective or strictly scientific evidence in favour of such an organ, such as we have in the lives of the saints, and in a lower degree, in the universality of the religions sentiment." A Candid Examination of Religion, p. 149, G. J. Romanes.

"Scientific men, as a class, are quite as dogmatic as the strictest sect of theologians. They professed to be agnostics, at the very time they were egregiously violating that philosophy by their conduct." Ibid., pp. 107–9.

But we can, I think, establish it as a law that "supernatural revelations" invariably take their colour from the preconceived ideas of the recipients. Probably, upon any hypothesis, they could do no otherwise.

One antecedent factor, in sudden conversions from the worldly to the religious life is often found in the physical effect of a severe illness; also those who have been rich in spiritual (?) experiences have often had indifferent health from childhood, with a liability to neurotic attacks. But there are too many exceptions to spiritual (?) experiences being the result of either temporary or permanent ill health, to enable us to say that such experiences are simply the result of disordered health, though they may be coincident with it.

I suppose that Socrates would universally be accepted as a type of moral and physical healthiness. Yet throughout his life he was subject to the promptings of what he himself calls an "inner, divine voice," which warned him if any evil were likely to befall him. In his sublime address to the judges who had just condemned him to die Socrates said that his "inner divine voice" had given him no warning of evil when he had left his house that day on which he was condemned to death, yet as it had always hitherto warned him of danger even on the most trifling occasions, he took its silence to mean that death was a good and not an evil. For even if death be a dreamless sleep, is it not a real and precious boon; and if it be as some say (and as Socrates himself hoped) only a passage from one state of being to another, to a place where all who have left this life are assembled, what greater good could man desire? To attain such happiness, Socrates would himself die many times.[2] Only in his own mind could the great philosopher seek for evidence of the one Supreme Being; for he could not believe in the popular theology which accepted gods who had committed acts which would have been disgraceful in the vilest of men. Therefore, it is not surprising that his "inner divine voice" did not profess to come from any supernatural power, but simply warned him of evil.

In the case of Mahomet we have a man of great strength of constitution, but one who was subject to strange attack, whether of epilepsy, catalepsy or hysteria is still a subject of doubt. What is certain is that he had a tendency to see visions, and suffered from fits which threw him at times into a swoon *without loss of inner consciousness.*

Through his intercourse with certain holy ascetics of the desert known as Hanifs, Mahomet became possessed with a profound sense of dependence on the omnipresent and omnipotent God. He withdrew to

[2] Apologia, XXXI, XXXII.

the solitudes of the bare and desolate Mount Hira, and meditated there with prayers and ascetic exercises. This state of things continued for many years, when in the month of Ramadan [which it will be remembered entails the severest form of fasting] the final revelation came which converted an illiterate Cameldriver[3] into one of the great religious teachers of the world. As he was repeating his pious exercises and meditations on Mount Hira the "divine voice" came to him. The angel Gabriel held a silken scroll before him, and bade him, though he could not read, recite what stood written in it. The words with which Gabriel had summoned him remained graven on his heart, and are found—as Mahomet at least imagined he heard them—in the 96th Sura of the Koran.

Mahomet returned to his wife Khadijah in great distress imagining he was possessed. But she comforted him and impressed upon him the belief that he had received a message from God.

Yet his doubts returned again and again, and reached a distressing height so that he was tempted to cast himself down from Mount Hira and this conflict lasted for two or three years.

Then one day he came to Khadijah in a state of great excitement exclaiming "Wrap me up, wrap me up!" (which was done when he fell into a fit, or swoon) and then the angel Gabriel appeared a second time, and revealed to him the Sura beginning "O thou enwrapped one!"[4] Henceforth there was no interruption and no doubt, the revelations followed without a break, and the Prophet was assured of his vocation.

The revolt of the idolatrous arabs against a creed of pure monotheism caused Mahomet at one time to yield to the temptation to humour them, and this temptation took the form of voice from the evil one, causing him to say in the pulpit that two of the heathen goddesses were sublime beings whose "intercession might be hoped for." His auditors were surprised and delighted, but the prophet went home disquieted. In the evening Gabriel came to him and Mahomet repeated the new *Sura*, whereupon the angel said "What hast thou done? Thou hast spoken in the ears of the people words I never gave thee." Mahomet now fell into deep distress fearing to be cast out from the Lord. But the Lord took him back to his grace and raised him up; the *Sura* of diabolical suggestion was erased, and the fury of the idolatrous party broke out with fresh violence. Mahomet was long and salutarily humbled by the remembrance of his temptation and fall, but he never abandoned faith

[3] It is still a subject of dispute amongst Moslems whether Mahomet couid read it seems, however, more probable that he could not.

[4] Sura, 75 of the Koran.

in his vocation, and through evil report and good report believed that he had a Divine message to deliver.

The earliest and simplest accounts of the life of Buddha [Siddhartha Gautama] all agree in describing the four visions which led to the renunication, by that religious teacher, of all the greatest goods the heart of man could desire. Some accounts make all four visions appear on the same day, others on different days, but all agree is making the four visions phantoms which were visible only to Buddha and his charioteer Channa. As Buddha was driving in his pleasure grounds he was struck by the sight of a man utterly broken down with age; on another occasion by the sight of a man suffering from a loathsome disease, and some time afterwards by the horrible sight of a decomposing corpse. Then an ascetic appeared walking in a calm and dignified manner, and the charioteer explained to the young prince the character and aims of the ascetics.

[5]"Subjectively though not objectively," says Mr. Rhys Davids, "these visions may be supposed to have appeared to Gautama," and undoubtedly at this time the mind of the young Rajput had become deeply stirred. The birth of his son did not deter Gautama from his resolution to lead an ascetic life, so that he might some days return to his loved ones not only as husband and father but as teacher and saviour; and on the night of the full moon in the month of July the young chief left his father's home, his wealth and power, his wife and child behind him, and with Channa as his sole companion, went out into the wilderness to become a penniless and despised student and a homeless wanderer. It would take too long here to attempt to explain the reasons which made the visions of Buddha naturally relate to the sorrows and emptiness of life, and not to the joys and promises of a future state. The great object to be attained was to put an end for ever to the cycle of births and deaths to which all human beings were considered subject, and to pass this life in such a manner that complete absorption into the World-Soul (Nirvana) should follow death.[6] The aim was *not* that conscious personal immortality, or that rejoicing in the love of a God who loves his creatures, which is the strong desire of the heart of Western peoples.

But in order to teach (what to him seemed) the way of salvation to his fellow creatures, Gautama made the greatest and most complete self sacrifice ever recorded of any human being; and for this great renunciation his memory cannot be too highly honored.

[5] T. W. Rhys Davids, Buddhism. Enc. Bri., Vol. IV.

[6] The Nirvana of Buddhism is simply extinction, op. cit., p. 434, and note 1.

Augustine, the great Bishop of Hippo may be taken as a type of the '*mens sana in corpore sano.*' He was a man who drank deeply of all joys, both of the body and the mind, which the cup of life could offer. Yet his great powers and commanding intellect did not prevent his hearing a "divine voice" which then and there influenced him to take up the religious life. It is true that the mind of Augustine had been deeply exercised by the search for truth, which ever seemed to elude his grasp. Plato and St. Paul opened the way for higher thoughts, and words of the latter were driven home with irresistible force to his conscience, as with his friend Alypias he was studying the Pauline epistles. The thought of divine purity fought in his heart, with the love of the world and of the flesh which were sore temptations to a man so admirably fitted to enjoy both. He burst into a flood of tears, and going out into the garden flung himself under a fig tree that he might give his tears full vent, and pour out his heart to God. Suddenly he heard a voice calling him to consult the scriptures. "Take up and read, take up and read." He left off weeping, rose up and returning to his house took up the volume from Alypius, and read in silence the words to be found in Romans XIII, 13th and 14th verses. Augustine adds "I had neither desire nor need to read further. As I finished the sentence the light of peace had poured into my heart and all the shadows of doubt dispersed. Thus hast Thou converted me to Thee . . . standing fast in that rule of faith which Thou so many years before had revealed to my mother." (Confessions, VIII, 30). This appears to have been the only occasion when a hallucinatory (?) voice was heard by Augustine, but its influence lasted for his whole life.

For that experience which points to the state commonly known as ecstacy,[7] I shall take the experience, not of a saint, nor of a prophet, but of a plain American citizen of our own day, a locomotive engineer who worked chiefly in Ohio and Indiana.

[7] Neoplatonism was a philosophical religion, in no way founded on any revelation real or imagined. Its great expounder Plotinus says simply of his own experience of "ecstacy" [that is of the sense of absorption into the Divine Beings] "I myself have experienced it but three times." But his pupil and disciple Porphyry says that on four occasions during the six years of their intercourse Plotinus attained to the ecstatic union with God.

It is surely contrary to the true scientific spirit to ignore this strong, overmastering instinct of the human mind towards union with some "Power, not itself, which makes for righteousness," and which appears in equal strength in the Hindoo, the Mahometan, the mediæval saints, the German mystics; and in the Neoplatonists—who had no "religious superstition" to influence them, and no hell to terrify them, but who were possessed only with the sense of an overwhelming need for union with the Divine.

This engineer, whose name was Skilton, was engaged with two other men in unloading a freight car. Just as he had lifted a barrel out to the platform he saw a "person standing" "on his right hand clothed in white" "and with a bright countenance"; and "putting his hand on my shoulder." "He said 'Follow me'." A long description ensues of the happy and beautiful place to which this personage led him; a place where the inhabitants did not converse by sound but knew each other's thoughts on the instant, and where he saw his mother, his sisters and his child. In fact the heaven it was natural for the percipient to to imagine, though—be it remarked not the heaven of harps and white nightgowns of popular theology—but such a heaven as the seer might naturally desire. Mr. Skilton describes the earth on his return as appearing to be seen from a great height, trees buildings, etc., gradually came into sight, and finally he reached the car which he had begun to unload, and then the guide vanished. "I spoke then" he says (just as I opened my watch and found it had been just twenty-six minutes that I had been engaged with that mysterious one) and said I thought I had left this world for good. One of the men said: "There is something the matter with you ever since you opened the car door; we havn't been able to get a word out of you" and that I had done all the work of taking out everything, and putting it back into the car. I told them where I had been and what I had seen, but they had seen no one. He adds: "This I count the brightest day of my life, and what I saw is worth a life time of hardship and toil. Being in good health, and busy about my work and my mind not more than ordinarily engaged on the great subject of eternal life, I consider this a most extraordinary incident."[8]

If it be said there is no corroboration of Mr. Skilton's statements neither is there any corroboration of any other similar statement; and if I have chosen his experience from among many others, it is simply to show that whatever religions (?) experiences occurred to persons in past times, *occur in precisely the same way now.*[9]

As an exemplification of the law that all spiritual (?) experiences are coloured by the prepossessions of the percipients, I may take the numerous cases of the alleged appearances of the Virgin Mary. The apparition to Bernadette Soubirons is only one amongst many, and in this case as in most others expectant attention is not a factor. The Virgin appeared to Bernadette when she was certainly not expecting to

[8] Communicated to Professor James of Harvard by Mr. Skilton.

[9] The exaggerations found in *legendary* times constitute a totally different branch of study.

see anything; and the crowds of pilgrims who have gone to Lourdes in a state of vivid expectant attention have seen nothing.

I have no space to describe the many instances I have met with of recent appearance of the Virgin Mary, under circumstances where there can be no reasonable doubt of the good faith of the percipients, nor do I recall any circumstances of religions exaltation accompanying these visions. Where pious Catholics see the Virgin Mary or the saints, equally devout Protestants see Christ or an angel, and both see spirits which they believe to be those of the blessed dead.

My own mind in regard to the question of religions experiences is in the state of suspense of judgment,—the state of "pure agnosticism" advocated by G. J. Romanes. [I need hardly say that this is *not* the agnosticism of scientific orthodoxy].

If in the present stage of our study of these phenomena we leave the stage of pure agnosticism, one of two hypotheses must, it seems to me, be accepted. Either such experiences are due (*in persons otherwise rational and capable of carrying out the business of life*) to some "organ of spiritual experience," or they are *all* in the domain of morbid psychology. The evidence is of a similar nature in all these cases, whether of Paul, or Mahomet, or Ansel Bourne, or Bunyan or Socrates or Mr. Skilton; the hallucination (?) visual or auditory or both is only manifest to the percipient, and to him appears objective. In the case of what are called "collective hallucinations"—which are not uncommon, we have also only the evidence of the percipients, who see what others do not see. The great difficulty in accepting the hypothesis that there is some real communication possible from "a Power not ourselves which makes for righteousness," is the frequent association of religions hallucinations with insanity. But if there be an "organ of spiritual insight" it can act only subject to the general functions of the organism, and must be liable to partake of its disorders; it can only be the frail machinery through which an Unknown Energy is acting.

So I leave the question, the most important, it seems to me, with which Science can deal. The history of Ansel Bourne is extracted from the Proceedings of the Society for Psychical Research.

ALICE BODINGTON.

ANTHROPOLOGY.[1]

Mr. Keane on Paleolithic Man.—Mr. A. H. Keane in his recent publication "Ethnology" takes serious exception to my denial of there having been a paleolithic period, and says "Paleolithic necessarily antedates Neolithic Culture." In what does this necessity consist? Why should Paleolithic precede Neolithic Culture? The names it is true signify old and new, but are at best arbitrary, being a suggestion of Sir John Lubbock to distinguish a supposed "chipped" from a "polished" stone period. Mr. Keane says "where there is a time sequence, the chipped stones being of ruder and simpler formation, naturally precede the more perfected polished objects." The proof of a "time sequence" is by no means a settled question, this assertion being negatived in one way or another by every writer on Archeology. The chipped stones are not "ruder" than polished stones, nor are they "simpler" in shape, material or facility with which the shape may be given. I have only attempted to discuss the subject from a technical standpoint and from the writings on the subject generally, from either of these points, however, or from both together I contend my position is sustained. Chipping stone is a more difficult mechanical process than grinding and pecking stone, it is more complicated in its minutiæ, involving, it is true, blows with a hammer, the difference being that the chipper's blow is of necessity more deliberate, slower and of necessity more accurate than the blow given in pecking. A doubt of the accuracy of this proposition may be solved by taking a flint and a diorite and attempting with any hammer to shape them. If ordinarily careful the diorite will be worn into shape, while on the other hand the chances are many to one that the flint is destroyed before completion.

Mr. Keane objects that "European archeologists are asked to reconsider their own conclusions." Undue weight being shown to have been given certain evidence, European archeologists owe it to themselves to reconsider their conclusions. Up to a recent period it was believed generally that to shape a Neolith or ground implement was more difficult than it was to shape a Paleolith or chipped implement, and such difficulty was used as the main evidence upon which to support the theory of a chipped preceding a polished stone age. Having been shown that the contrary was the case, one would presume

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

European archeologists would seek to examine their error without being forced to do so. Mr. Keane says "There is necessarily a time sequence wherever the two cultures have been developed." The mistake the author falls into is in declaring that to chip a stone or to grind and peck a stone constitute two cultures, for experiment proves the contrary; try to chip a diorite and you only shape it by powdering the surface, for it does not chip, try to batter a flint and it breaks through the ordinary lines of cleavage and is destroyed, for it does not peck.

Again the author of Ethnology says "that until it is shown that fire arms are as old as paleoliths, no European archeologist will ever believe that polished implements are as old as the chipped stones." Though this will hardly pass as scientific argument we would say, here again Mr. Keane is in error, for instances of chipped and polished stones being found in the same layer with fossil bones has been recorded on too many occasions to leave room for doubt to one who would decide on written authority alone. Up to this time Mr. Keane has argued in favor of the simpler process preceding the complex, but here he says "it is a fallacy to suppose that the easier process comes first," and instances "transport by wheeled vehicles or by steam as immeasurably easier than pack animals." If we are to construe the word "easier" as being synonomous with "simpler" it opens a new field of argument to assert that the complex precedes the simple in machinery, or that the machine of many parts is the ancestor of the machine of few parts, this proposition will not meet with many supporters.

My views concerning the mechanical status of primitive peoples has been formed solely from experiment with primitive tools and reading the literature of archeology. The technology of archeology appears to be little understood in Europe, its importance being almost ignored, as is evidenced by the apparent inability to grasp the plainest mechanical propositions. The results of my experiments are sustained by my field discoveries as they are by research in the library, and considered from any of these positions the fact that an identical mechanical culture produced, chipped or polished stone appears to be indisputable. The earliest cave remains show man to have made the best use of material at his command. Where he had flint he chipped it; if on the contrary he lived in a region of metomorphic stone he would of necessity hammer it into shape; if horn and ivory were plentiful he would saw and grind it. If one will make experiments with primitive implements in reproducing primitive work they will not fail to appreciate the correctness of the views here expressed. As illustration take

material from Magdalen Cave, from Les Eyzies, from St. Acheul, from Moustier, from Chelles, even from Cissbury or from where one will, and try to make from it arrow heads of a different type from that usually found in the locality from which the material is brought and examine the result. One finds that flint chips within certain limits, for it depends even more on the material than on the workman as to the shapes the nodules work into. The difference in the tool with which the stone is worked is secondary to the texture of the stone. One of the best illustrations of this is in the obsidian spear heads from Easter Island. They are of a gritty texture, extremely rude, fully as rude as the rudest paleolith, and are chipped almost entirely from one side. Try to improve the shape of one of these implements and rude as they are, failure is the inevitable result; try to chip it from the wrong side and it breaks through and destroys the specimen, the best and most expert workman cannot improve its shape. Take, however, one of the obsidians from Mexico of even texture and to shape it in most graceful form is most easy, but with such material it would be almost impossible to imitate the rough arrow heads of Easter Island. The same stone varies enormously in its fracture in different layers, yet archeologists do not appear to have noticed the fact.

Only a few years since it was argued that paleolithic man was primitive man, man low in the scale of human development, to-day paleolithic man is apparently only primitive as a flint chipper, but an artist as a bone or ivory worker; the fact that technically considered the work necessary to shape a so-called "Baton of Command," itself probably a chipping tool, was identical with the chief work on the neolith (grinding) does not appear to be appreciated by those who believe in a paleolithic period. Five years ago or little more it was hotly denied that pottery belonged to the paleolithic period, and this was insisted upon until the accumulation of proof was so overwhelming that by many it was admitted, yet even now many deny that pottery is as ancient as many of the paleolithic cave strata.

J. D. McGuire.

Cave Exploration by the University of Pennsylvania in Tennessee.—*Preliminary Report.*—To learn that the remains of Plistocene Man have been abundantly found in the caves of Europe; that equally significant remains of later savage, barbarous and civilized peoples have been similarly discovered in the caves of Europe, Asia and Africa; and that the remains of the Indian and the recent White Man have been found in caverns in North

America; warrants the supposition, that in the subterranean floor deposits of the new world, the problematic existence of Plistocene Man might be soonest and easiest demonstrated, while with hardly less ground we may urge as valuable testimony in the American region the absence of such remains in significant underground shelters. Not unreasonably such absence, occurring invariably at these immemorial halting places of men and animals, might indicate that Plistocene Man had never existed in the adjacent regions.

By this course of reasoning and investigation the University of Pennsylvania has sought to solve definitely the question first to attract and last to puzzle American students—How long has Man existed in the New World? Striving to limit the speculations of archæologists, the work has proceeded by degrees to reconcile with geology their study of pre-Columbian peoples, which, fascinating as it is, has lacked thus far subdivisions, landmarks and starting point, while an effort to eliminate, through the investigation of significant caves, one region after another from the field of search, has sought to narrow the area of possible discovery from the point of view explained. Having shown on the one hand that certain caverns like the fissure at Port Kennedy, (right bank of Schuylkill River, 3 miles below mouth of Perkiomen Creek, Montgomery County, Penna.,) containing in large quantity the remains of Plistocene animals without relics of Man, are geologically ancient, on the other hand, a fact of much significance has been demonstrated for the first time, namely, that a considerable number of other caves are modern, since their floors, well supplied with the refuse of Indians and later White Men, below which remains of geologically older peoples would not have been lacking in Europe, have failed to reveal any relic of Plistocene Man.

In these several instances the geologically modern remains (human) and the geologically ancient remains (animal) have lain apart in distinct caves, and hence less availably for comparative study, but the recent expedition in Tennessee, resulting in the examination of three caves in which the old and new deposits lay in juxtaposition, has enabled us to push the question farthur by studying the relation between the ancient and modern strata where, at their point of contact, it was most significant.

More broken and scattered even than at the remarkable tomb of extinct animals at Port Kennedy, were the remains of the Tapir, Peccary, Bear and smaller Mammalia at Zirkel's Cave, (left bank of Dumpling Creek, about 5 miles above its mouth in French Broad

River, Jefferson County, Tennessee,) visited by Professor Cope in 1869. Dislocated as before after the flesh had rotted, the bones were crushed by a force which had split them into fragments, and were deposited with a mass of clay mixed with lime, which filled the descending cave. Hardened finally into breccia not easily broken with the pickaxe, this bone bearing earth had disappeared at many points to make room for a deposit of cave earth containing the remains of the Rattlesnake, Woodchuck, Opossum, Rabbit and Cave Rat. It is the important relation of this latter modern earth, with its bits of mica and Indian pottery, to the older breccia that will constitute the material for a final report.

Previous examination, in 1893, at the Lookout Cave, (left bank of the Tennessee River, one-quarter of a mile below Chattanooga Creek, Hamilton County, Tennessee,) had revealed the bones of the Tapir and Mylodon in the lowermost zone of a floor deposit of Indian refuse, and upon the recent expedition the cave earth with its "culture layer" was entirely removed for 58 feet inward from the entrance to settle beyond doubt the relation of these fossils to the Indian remains resting upon them. At this significant spot, where again the Plistocene and recent deposits lay in contact, and where the specimens found were labeled according to their position, whether from the black (modern) earth above or the yellow (ancient) earth below, a completed examination should decide whether Man had or had not encountered the Tapir and Mylodon in the Valley of the Tennessee.

After a visit to "Indian Cave" on the Holston River, Carrol's Cave, and the Copperas and Bone Caves, near Tullahoma and Manchester, Tennessee, a new set of conditions was presented at Big Bone Cave (1 mile from the left bank of Caney Fork and about 2 miles above its mouth in Rocky River, Van Buren County, Tennessee.) There the bones of the Gigantic Fossil Sloth (*Megalonyx*,) still retaining their cartilages, were exhumed from a dry deposit of the refuse of Porcupines and Cave Rats, mingled with fragments of reeds used as torches by Indians in a gallery 900 feet from the entrance, thus presenting us in the final summing up of this strange evidence a new notion of the relation of the modern Indian to this extinct animal, whose remains outnumber all its fossil contemporaries at Port Kennedy.

Thanks are due to Dr. William Pepper, to the Board of Managers and to Professor E. D. Cope for their kind co-operation in the expedition thus finished, which, at a cost of $300, has presented the Museum with the specimens now under examination. These, if not attractive,

are important. For Paleontology they mark in the bone breccia of Zirkel's Cave, a distinct stage in the Plistocene series, while for Anthropology they represent data which account for the presence of Man together with the bones of the extinct *Megalonyx*. They explain the relics of savages and the remains of Plistocene mammals at two caves situated in the Eastern Valley of Tennessee at a height of about 600 to 700 feet above the sea and within earlier reach of an overwhelming ocean in Champlain time, and again at a third cave, which, 300 feet higher on the continental floor and looking westward from the slopes of the Cumberland table-land, stands for that part of the Appalachian region whither animals and Man (if he existed) might have found convenient refuge when lower areas sunk, as is alleged, beneath the level of the invading waters.—HENRY C. MERCER.

Aldie, June 4, 1896.

SCIENTIFIC NEWS.

The proposed general synopsis of the Animal Kingdom (Das Thierreich) to be issued by the German Zoological Society, is one of the greatest undertakings ever planned in the line of bookmaking. It is proposed to give a short general account of each group, and following this is a synopsis of all existing forms, including those which have recently become extinct. The general editor of the whole series is Prof. Franz Eilhard Schulze of the University of Berlin, and he is assisted by the following department editors: Prof. O. Bütschli, Protozoa; Prof. C. Chun, Coelenterata; Prof. M. Braun, Plathelminthes; Prof. J. W. Spengel, Vermes; Dr. W. Kobelt, Mollusca; Dr. W. Giesbrecht, Crustacea; Prof. R. Latzel, Myriapoda; Prof. F. Dahl, Arachnida; Dr. H. Krauss, Orthoptera; Mr. A. Handlirsch, Neuroptera, Hemiptera; Dr. H. J. Kolbe, Coleoptera; Prof. C. W. Della Torre, Hymenoptera; Dr. A. Seitz, Lepidoptera; Prof. J. Mik, Diptera; Prof. F. Blochmann, Brachiopoda; Prof. E. Ehlers, Polyzoa; Prof. J. W. Spengel, Tunicata; Dr. G. Pfeffer, Fishes; Dr. O. Boettger, Batrachia and Reptila; Prof. A. Reichenow, Birds, and Prof. L. Döderlein, Mammals. These will be assisted by a host of collaborators for special groups, and the names of these, as far as announced, assures us of the

most authoritative treatment. The whole work will be of enormous extent, and it is estimated that it will take twenty-five years for its completion. It will be issued in parts of an average size of 48 pages, and the price charged to subscribers for the whole series will be 70 pf. (17½ cents) for each "signature" of 16 pages; single subjects will be sold at an advance of ⅓ above this price. It is estimated that the Flatworms will occupy 4 parts; the Crustacea 11, the Hymenoptera 13; the Mollusca 15, the Reptiles 3, and the Birds 16. The total will be over 120,000 pages, and the series, when complete, in large octavos, will occupy not far from 30 feet of shelf room. The publishers of the series are the well-known R. Friedländer & Sohn, of Berlin, and they have already issued a sample number, embracing the group of Heliozoa, treated by Dr. F. Schaudim, of Berlin. This occupies 24 pages. It is expected to begin regular publication with the year 1897, and the parts will be issued as fast as possible, without regard to their sequence in the whole work.

It will probably be interesting to know that a party of five from Cornell, under the direction of R. S. Tarr, will accompany Lieut. Peary on the trip to Greenland this summer. The party will be so constituted that the results will cover the several fields of natural history, although the main object will be the study of geology, and especially glaciation.

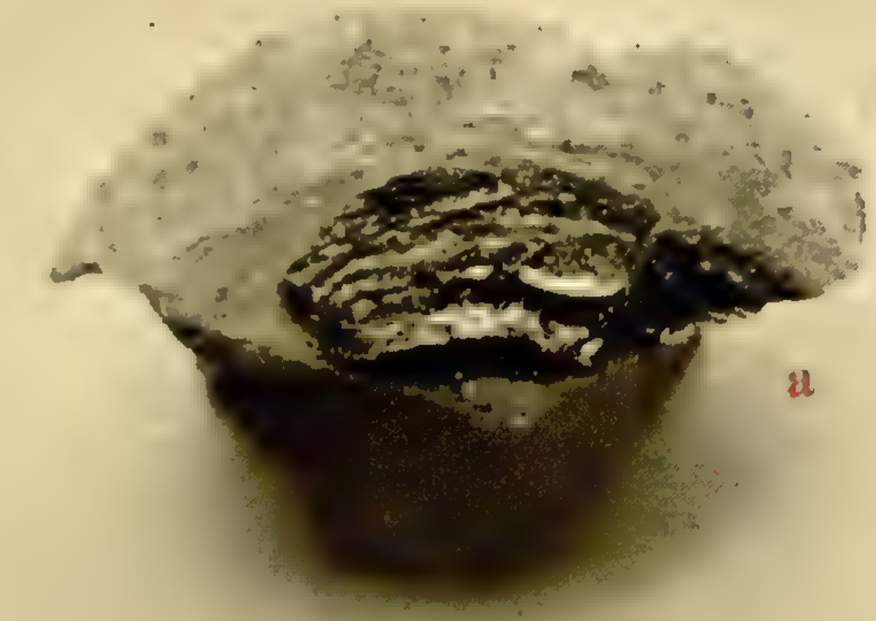
The next meeting of the American Microscopical Society, August 18–20th next, is to be held in the Carnegie Library Building, Pittsburg. The local Committee on Arrangements is organized: C. C. Mellor, Chairman; Mayness Pflaum, Secretary and Treasurer, and C. G. Neilnor, Chairman Finance Committee, either of whom will be glad to give members and others desiring to attend all necessary information. As soon as sufficient arrangements are made, special announcements will be mailed to all members.

Dr. R. Wagner, of Strassburg, has been appointed assistant in Vegetable Physiology in the University of Munich, and Dr. A. Y. Grevillius, of Upsala, assistant in Botany at Minster.

The Royal Belgian Academy of Sciences has recently elected Professors E. Strasburger, E. D. Cope, E. J. Marey and Sir A. Geikie to honorary membership.

Prof. M. Treub, Director of the Botanical Gardens, at Buitenzorg, who has been spending some time in Europe, has returned to Java.

Dr. A. Fleischmann has been advanced to the position of Extraordinary Professor of Zoology in the University of Erlangen.



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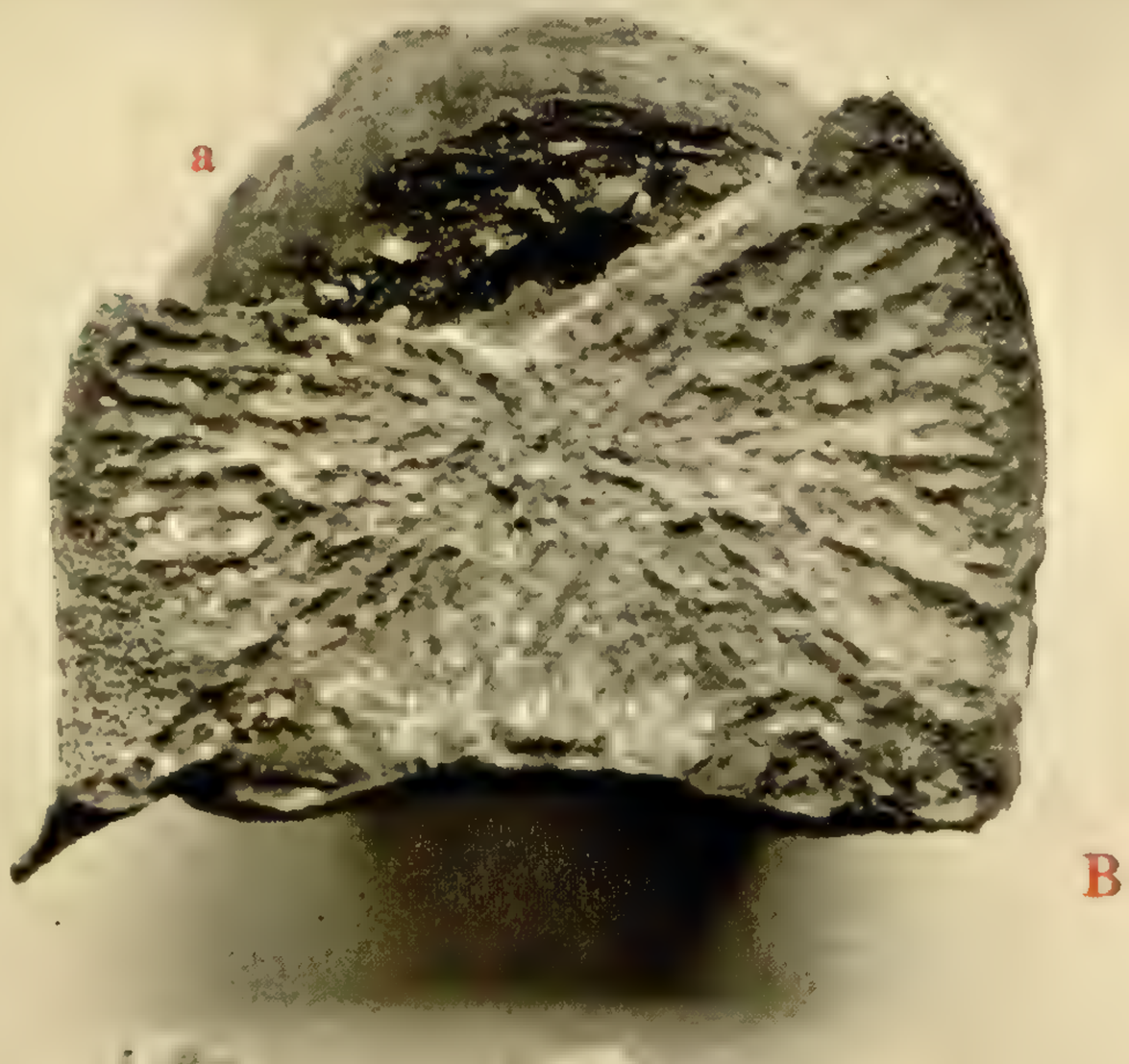

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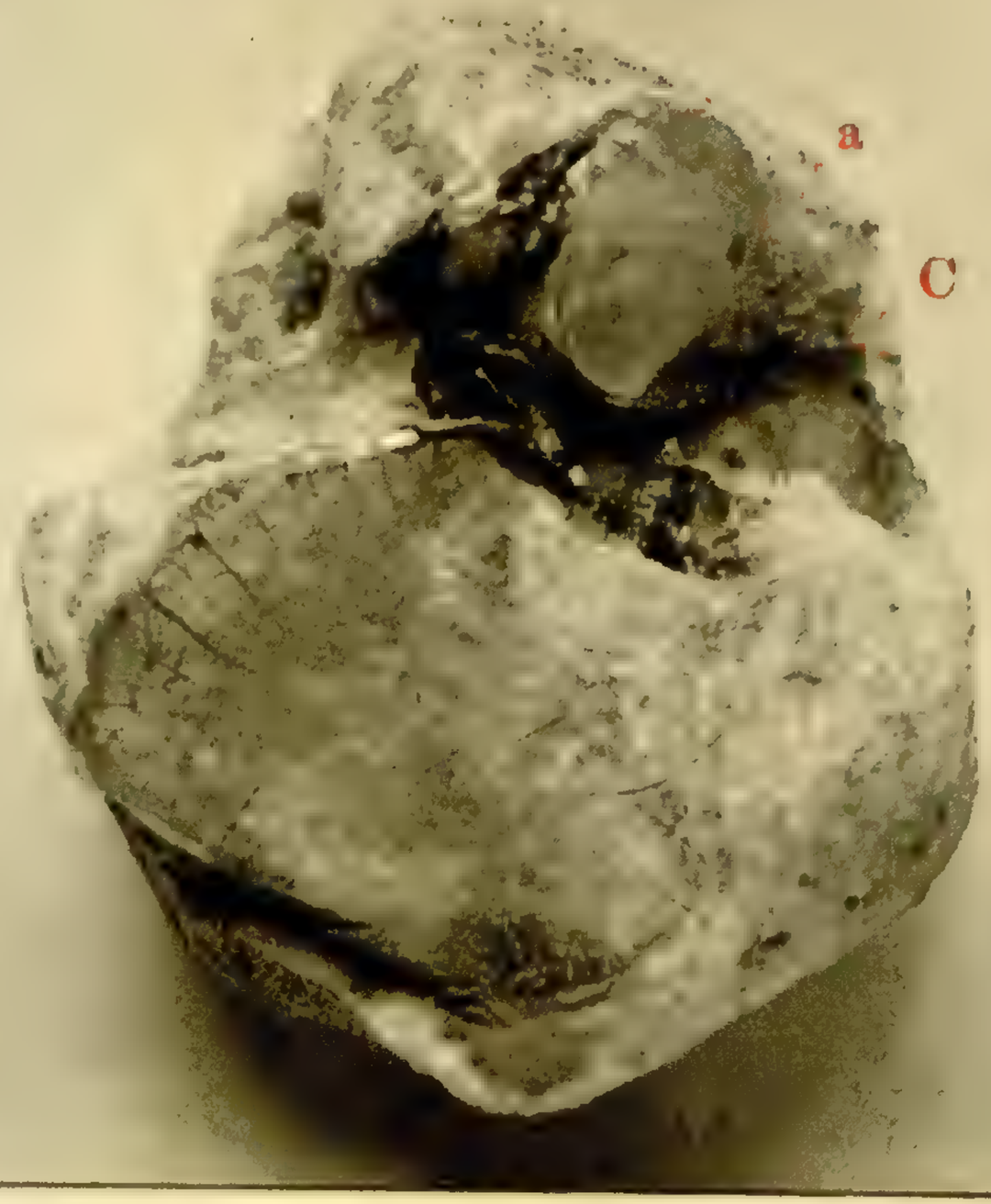

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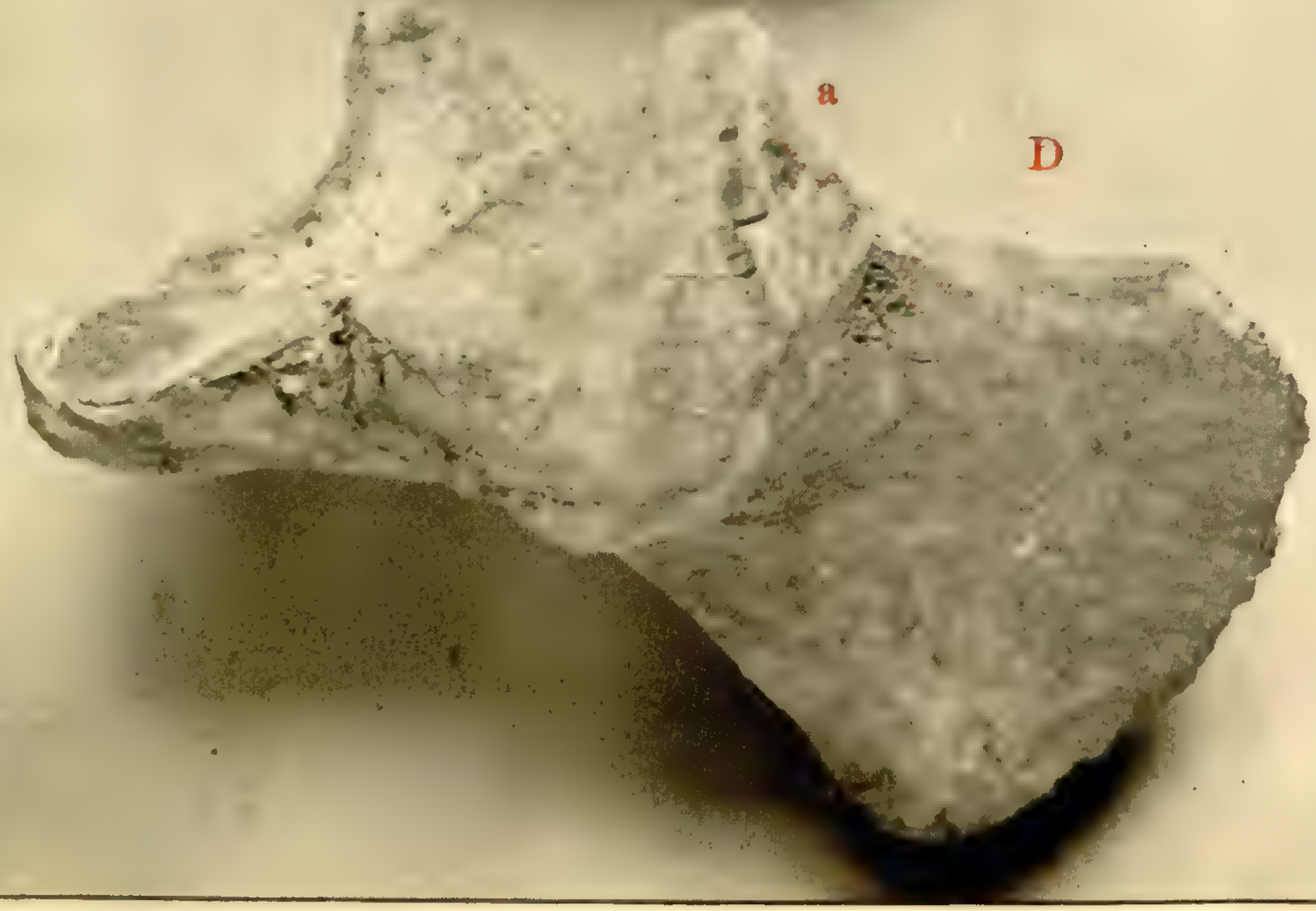

D

Megalonyx jeffersonii with articular cartilages at a. A, B, Vertebral epiphyses. C, Astragalus. D, Calcaneum.

The Royal Irish Academy recently elected Sir Joseph Lister, T. G. Bonney and Sir W. H. Flower to honorary membership.

Dr. G. A. J. A. Ondermann, Professor of Botany in the University of Amsterdam, has retired on account of his great age.

Mr. A. Lawson, botanist and director of the Cinchona plantations in the Madras district, died at Madras, February 14th.

Prof. D. Barfurth, of Dorpat, goes to the University of Rostock as Ordinary Professor of Comparative Anatomy.

Dr. H. Ph. Foullon von Norbeck has been appointed Chief Geologist to the Austrian Geological Survey.

Dr. G. Rörig, of Berlin, goes to the University of Königsberg as Extraordinary Professor of Zoology.

Mr. H. M. Drummond-Hay, ornithologist and ichthyologist, died recently at Perth, Scotland, aged 82.

Dr. J. Briquet, of Genoa, has been appointed Director of the Delessert Herbarium in that city.

Mr. J. H. Ashworth succeeds Dr. Hurst as Lecturer on Zoology in Owens College, Manchester.

Mr. A. S. Olliff, entomologist, died in Sydney, N. S. W., December 29, 1895, aged 30 years.

W. L. Sclater, of Eton College, goes to Cape Town as Curator of the South African Museum.

Dr. Thilenius has been made privat docent in Anatomy in the University at Strassburg.

Dr. C. Herbert Hurst, of Manchester, goes to Dublin as assistant to Prof. A. C. Haddon.

Dr. A. Smirnow, of Kazan, goes to the University of Tomsk as Professor of Histology.

Col. Plunkett has been elected Director of the Science and Art Museum in Dublin.

Dr. A. Bogdanof, Professor of Zoology in the University of Moscow, died in April.

Dr. G. Fatta has been appointed assistant in the Botanical Institute at Palermo.

Dr. R. Oestreich is now privat docent in Anatomy in the University of Berlin.

Dr. Szymonowicz is privat docent in Histology in the University of Cracow.

Dr. Gruner, of Jena, goes on an expedition to Togo, German West Africa.

Prof. Ph. C. Sappey, the well known anatomist, died in Paris, March 13th.

A. Duvivier, student of Coleoptera, died in Brussels, Jan. 14, 1896.

F. Ludy, coleopterologist, died March 1st.

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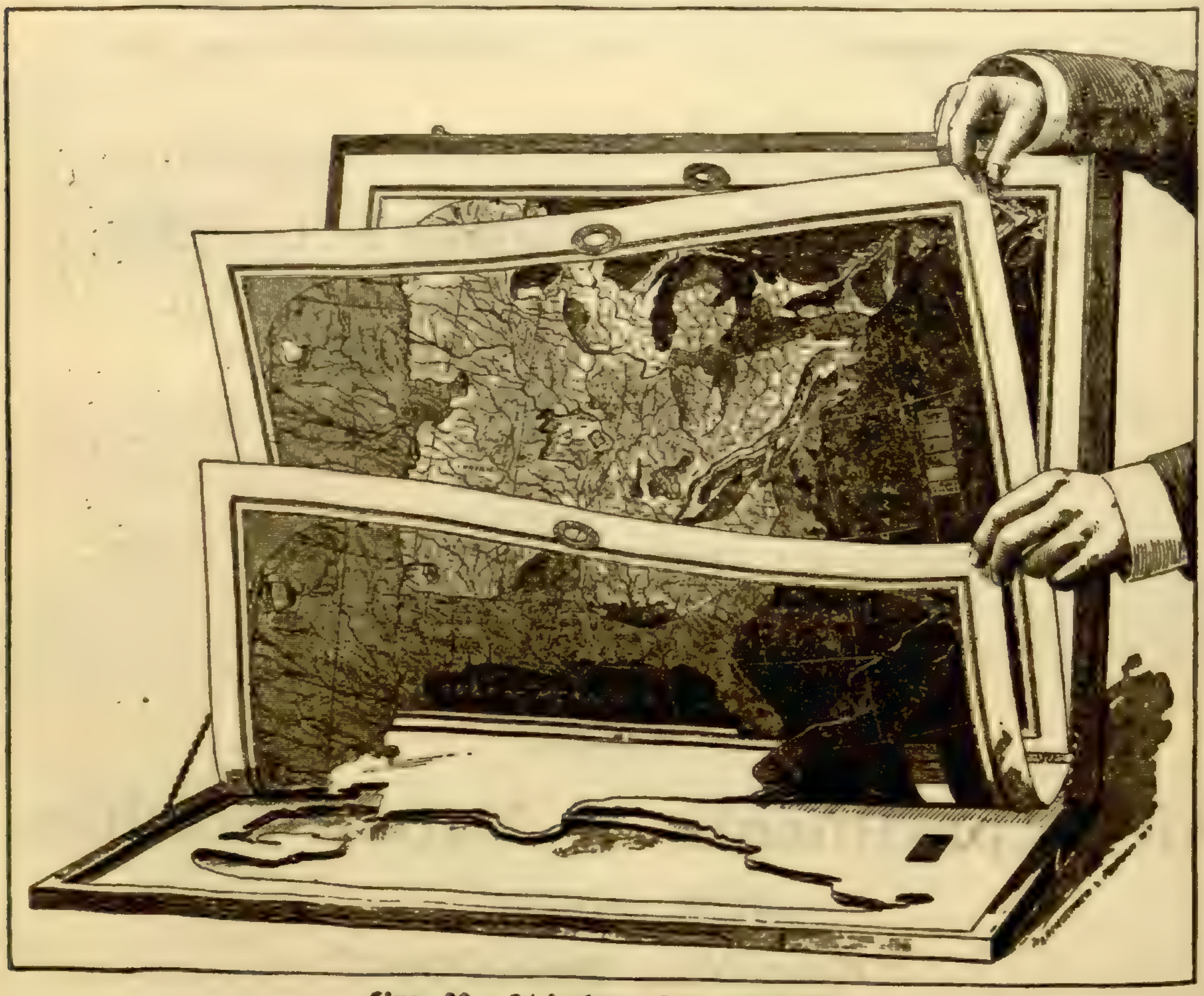

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Rev. Wm. C. Winslow, D. D., L. L. D., Egypt.
Prof. T. F. Wright, Explorations in Palestine.
Henry W. Haynes, Paleolithics and European Archaeology.
Dr. A. S. Gatschett, Indian Linguistics.
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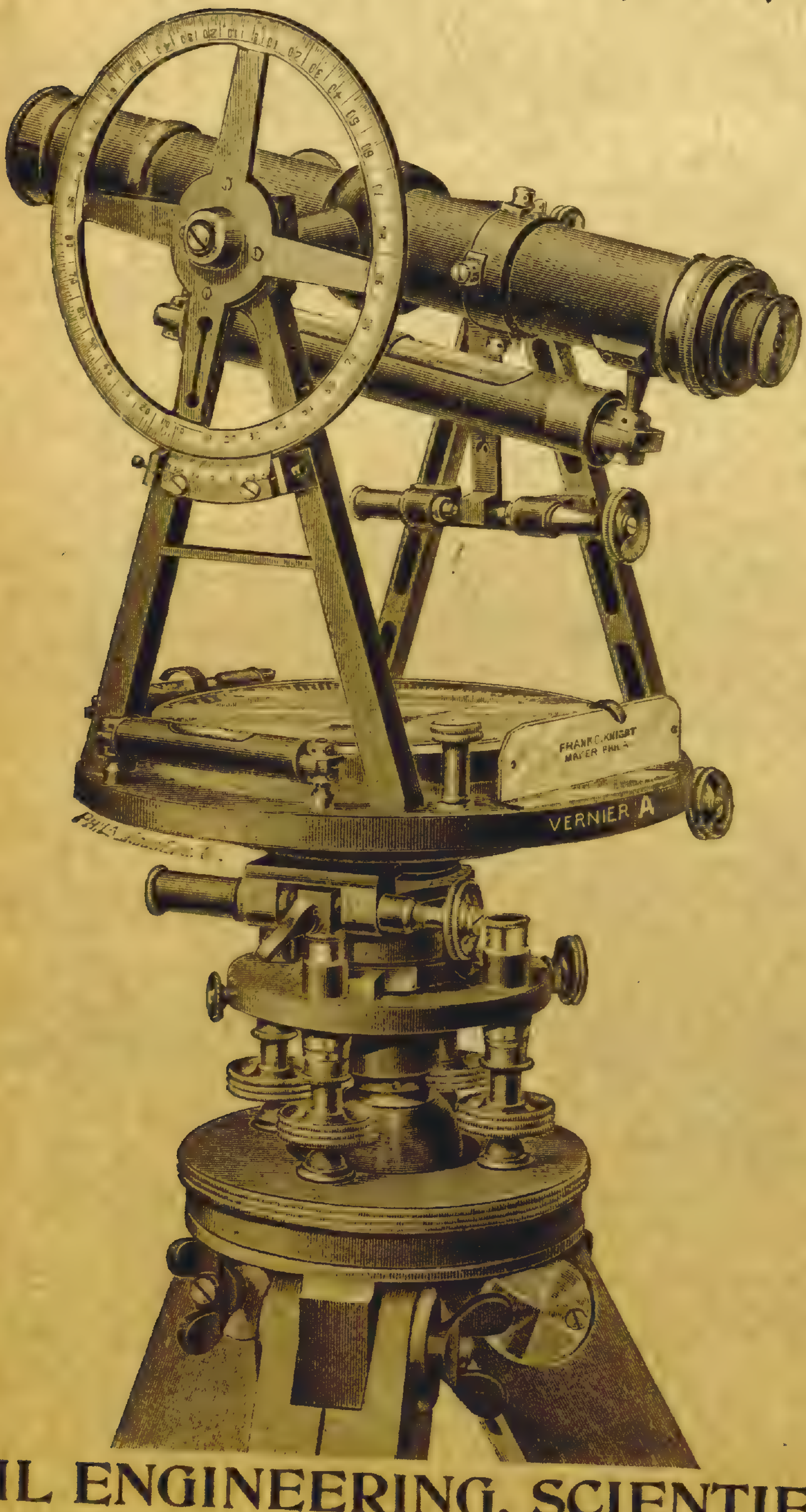

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Vol. XXX. AUGUST, 1896. No. 356

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PLATE XII.

Sumerian Tablets from Nippur. From Hilprecht.

THE
AMERICAN NATURALIST

VOL. XXX. August, 1896. 356

THE OLDEST CIVILIZED MEN.

BY E. D. COPE.

Recent explorations in Babylonia have given us much information as to the characters and customs of the oldest civilized people of whom we have any knowledge. The earlier explorations were conducted by M. de Sarzec, French consul at Bagdad, and the report of his work was issued in a magnificent folio in joint authorship with the distinguished anthropologist of Paris, M. Leon Heuzey, beginning in 1889. A little later the department of Archeology and Paleontology of the University of Pennsylvania sent out Messrs. J. P. Peters and J. H. Haynes to make excavations further south in the Euphrates plain. They selected Nippur or Nufar as the point of research, and work has been continued there from 1888 to the present year. The climate of this place is very trying, and the character of the people dangerous, but Mr. Haynes on whom much of the actual labor fell, obtained an amount of material which in quantity and quality equals that obtained by the museums of London and Paris.

The Philadelphia material has been investigated by Dr. Herman Hilprecht, Professor of Assyrian and comparative Semitic Philology in the University of Pennsylvania, and he

has published two memoirs of great interest in the transactions of the American Philosophical Society, the second of which was issued in the present year. From this memoir and the previous one of De Sarzec and Heuzey I compile a few facts regarding the physical characters and habits of the earliest inhabitants of Chaldea, the Sumerians or Accadians. The information on these points is obtained largely from statues and picture-carvings on tablets of a dark limestone, found by De Sarzec at Tel-lo, and by Haynes at Nippur. The figures of animals of known species are so characteristic as to prove that the artists possessed a true eye for form. We may infer that their delineations of man are equally accurate, and that the conspicuous characters which they exhibit are trustworthy delineations. The general resemblance between the features depicted show that we have to do with an interesting and peculiar race.

In the numbers of the NATURALIST for January and February, 1893, Mrs. A. Bodington gave our readers a sketch of the Sumerian question. She followed the belief which had gained currency at one time, that these people were of Mongolian origin. Others have suggested that they were African. The drawings and statues described by Heuzey and Hilprecht show that these ideas were quite unfounded. I reproduce one of the latter from Hilprecht (Plate XVI, l. c.), which is known as the stele of Ur-Inlil. Ur-Inlil was the high priest (or padesi) of Nippur, and he is represented as making an offering to some god on the upper half of the drawing. On the lower half a goat and a sheep are followed by two men, one of whom carries a vessel on his head, the other carries a stick (Plate XII). Another tablet from Nippur displays the same kind of men, and they are also represented on eleven tablets figured by De Sarzec and Heuzey from Tel-lo.

That these represent a race advanced in civilization is clear. They built temples and palaces on huge plateaus constructed of brick. They carved statues and vessels and made pottery. Especially they left records of their history on numerous cylinders and tablets of clay of which many have been preserved. They formed organized armies armed with spears, bows, and shields. What relation did these people bear to the people

of Nineveh whose monuments were revealed to Europe by the labors of Rawlinson, thirty-five years ago? Heuzey declares them to have been older than the Assyrians, and this position is proven by Hilprecht, who believes their earliest king whose name is preserved in the records of Nippur, Enshagsagana, to have lived 4500 B. C. Many kings intervened between him and Sargon I with whom Assyrian history for a long time commenced. These people were predecessors of the Assyrians of Nineveh, and gave them their cuneiform characters, but they differed from them in customs, and to some extent in language. One marked difference of custom, was the fashion of shaving the hair from all parts of the head excepting the eyebrows. Everyone knows on the contrary that the Ninevites took great pride in their hair, and that both on the calvarium and face it was curled and arranged with great care. The figures also show that the Sumerians did not practice circumcision as most Semitic and some other races have done.

The shaving enables as to get a pretty good idea of the form of the head and face. The skull is oval, rather long and flat, and probably mesaticephalic. The jaws both upper and lower, are remarkably small, giving an extreme orthognathic type The nose is remarkably long, prominent and curved, with a good bridge. The eyes are large, horizontal, and not bridled. The cheek-bones are not large, and in the supposed gods, where the hair remains, and in a few other unshaved portraits, the beard is abundant, and the ends of the hair of the calvarium curled up. The figure of the body is robust, broad and rather short. The extensor muscles, i. e. gluteus, quadriceps, and gastrocnemius are well developed.

From the above it is evident that no thought of Mongolian (=turanian) or Ethiopian relationship can be admitted. After a study of some of the least characteristic heads broken from statues, M. Heuzey remarks, that "the evidence is not sufficient to demonstrate the existence of Turanians in Chaldea." These people are clearly of the great Indoeuropean subspecies of man (*Homo sapiens caucasicus*), so that the question reduces itself to one of the determination of their race position. Are they Aryans, or Semitics? using these two terms as covering all the forms of the greater subspecies to which they belong. In the

determination of these minor divisions of man, physical characters begin to fail us. We can only say that if the term Aryan is used for the western peoples generally, the Sumerians differ from them in the direction of the Semitics by their large oval eyes and hooked noses. On the other hand, the small and delicate jaws are not features of Semitic peoples. But the people of Persia or Iranians, hold very much this intermediate position between the two peoples. We scarcely know the shape of the jaws and chins of the Ninevites for they are never shaved. So far as the visible features go they resemble the Sumerians. It is on all grounds to be supposed that the people of Nippur and Tel-lo are the primitive Aryans of the Iranian or Persian race, and ancestors of the Ninevites.

In any case it is evident that we have in these most ancient of civilized people, a type of man as high as any that has since appeared from the point of view of physical evolution. The extreme orthognathism; the prominence of the nose; the reduction of the cheek bones, the full beard; and the well developed extensor muscles of the leg, prove this. *Homo sapiens caucasicus* had reached his full characters on the plains of the Euphrates 6400 years ago.

The relation of time and race of the oldest civilizations to the prehistoric peoples, is a problem which will doubtless be solved in time. Did the Neolithic people exist in Europe contemporaneously with the Sumerians of Chaldea? The only light that can be thrown on the question is as follows. The Sumerians were not stone people, but bronze people. They had no knowledge of iron. No search has been made for the remains of animals which were their contemporaries, but several species are clearly represented on their sculptures. The most common are the lion and the ox (*Bos taurus*, not the buffalo). There is a good drawing of a gazelle in the collection of the University of Pennsylvania. The goat and sheep represented on the accompanying Plate XII, are species now existing in Persia. The goat is near the *Capra ægagrus* of the mountains of East Persia, the ancestor of the domestic goat; and the sheep is apparently the wild sheep of the same region *Ovis vignei*. So from a paleontological point of view, the Sumerians were quite modern.

ON THE ROLE OF ACID IN THE DIGESTION OF CERTAIN RHIZOPODS.

By John C. Hemmeter, M. D.,[1]

In the "Annales de l'Institut Pasteur," for 1890 and 1891, there are two papers by M. Felix le Dantec on "Researches on the intracellular digestion among the Protozoa," which are detailed accounts of systematic experimentation concerning the occurrence of acid in the digestive vacuoles of Protozoa.

In 1889, E. Metchnikoff published a discussion of the reaction of plasmodia to ingested litmus, also in the "Annales de l'Inst. Pasteur."

Miss M. Greenwood and E. R. Saunders, in the "Journal of Physiology," Vol. XVI, 5 and 6, 1894, have published an exhaustive account of the function of acid in Protozoan digestion, of which the following brief abstract is considered necessary before proceeding to the original part of this report.

It was found that while these protozoa ingest solid matter constantly and promiscuously, such matter has a determinate fate. If it is innutritious it is ejected after lying in contact with the animal's substance for a length of time which varies with many changing conditions. Nutritious matter, on the other hand, during enclosure in food vacuoles undergoes profound change, and this change is effected by something passed out of the protoplasm into the vacuole, acting in a fluid medium and by its presence making that medium deserving of the name "secretion." In Actinospaerium, also, and in *Amoeba proteus*, digestion in like manner is effected, not by direct contact with the acting protoplasm but by some constituent of a fluid, the formation of which the presence of food alone is potent to bring about. These protozoa depend upon the solution of proteid for nourishment. Starch undergoes no digestive change, and the value of ingested fat globules is doubtful.

The following is a report on the role of acid in these digestive vacuoles. For method of observation, it may be briefly

[1] Phil. D. Etc. Baltimore, Md.

stated, that plasmodia and Vorticellidæ were watched for periods which varied from one to fifteen days; large plasmodia were isolated or preserved in concave slides. Even on plane slides the pressure of the cover slip was slight enough to allow of the emission of short pseudopodia in planes at right angles to the plane of extension of the slide and the animals, by means of pipettes, were transferred to fresh water daily.

In a synopsis of the work of Greenwood and Saunders, in a previous report bearing on this matter, in the Journ. of Physiol., the changes undergone by litmus, Congo red and alizarin sulphate, and the solution of the globoids of aleurone grains, which are composed of a delicate nitrogenous capsule enclosing pure calcium and magnesium phosphate, were described. It was emphasized that the outpouring of acid is unaccompanied by any digestive change on nutritive matter; ingesta may indeed be stored for many hours in vacuoles before they are dissolved, or digestion may follow rapidly on ingestion. But the formation of the digestive vacuole, whether immediate or delayed, is preceded by the development of acid reaction and followed by its diminution. Bearing in mind that litmus is changed from blue to red not only by free acid, but also by unsaturated compounds of acid with the products of digestion, *i. e.*, acid salts. And that Congo red changes to blue in presence of free acid only. It is apparent that the diminution of acid in a digestive vacuole is at first due to a combination with the products of digestion, for at this stage any litmus accompanying ingesta is still red, while Congo red has reverted to that tint from blue. Here free acid is absent but acid salts are present. But later on the vacuoles and ingesta, reddened by litmus, become violet and blue so that finally acid and acid combinations are alike absent. That the acid is at one time free is indicated unmistakably by the striking development of violet colors in solids stained with Congo red. Now as the amount of acid present at any moment must be very small, and this being so, that the change in Congo red should be speedy and striking suggests that it is an inorganic acid but it is probable that to emphasize such an inference would be hasty.

In most of the existing records of Protozoan digestion there are indications that the process shows irregularity in its outset and progress. It is not easy to foretell the immediate fate of ingested matter though of its ultimate fate there may be little doubt. There may be marked inhibition of digestive activity even after free ingestion. In plasmodia ingested nutrient matter may be actually discharged after very imperfect digestion. One of the most puzzling phenomena, however, that has been described by all observers in this field, has been termed by Greenwood and Saunders the stage of storage. This process consists in the preservation of ingested food masses, which on first enclosure have been surrounded by liquid within a vacuole, in a shrunken seemingly very acid state. At times 100 non vacuolate, acid ingesta may lie within the substance of a vorticella, whilst active digestive solution is going on in other food vacuoles at same time.

The storage of nutritious ingesta for hours and days in a condition in which acid indicators give evidence of an acid condition, whilst the same kind of nutritious material will undergo rapid digestive solution in an adjacent vacuole, naturally excites one's curiosity. For a long time I had been looking in vain for some explanation of this phenomenon when an accident gave opportunity of viewing it in a new light.

The plasmodia of a large mycetozoon, most probably *Lamproderma scintillans*, had been under observation for about three weeks. Some of these amoeboid organisms were so large as to more than cover the field of vision when objective D and apochromatic eye-piece, No. 4 of Zeiss were used. They showed a habit of devouring everything in their vicinity in the ditch water in which they were cultivated, as a result of which they were at times so filled with debris that no accurate observations were possible. It was planned to transfer them gradually by pipettes into clearer and clearer water and by starvation compel them to rid themselves of the dirt they contained. This proved successful and after 8–18 days of transferring the plasmodia were in practically clear water, free from algæ, infusoriæ, gregarines, bacteria, etc., and the usual

fauna and flora of ditch water. It was a surprise to find that dried egg albumen stained or ingested with litmus and Congo red, under these new conditions, was as a rule promptly dissolved in the vacuoles, taking from 5–24 hours for completion of the digestive act. The same occurred with stained globoids of aleurone grains of ricinus and with stained torulæ. These experiments were repeated many times on many different individuals, and though food ingesta were occasionally observed in a stage of storage, this was the great exception.

The scarcity of storage vacuoles in such plasmodia that had been kept in clear water for nearly a week and given opportunity to disgorge the debris with which they were loaded was conjectured might be brought about by two factors:

(1) The first was that the process of clearing and transferring them to distilled water (in which they do not thrive as well as in Pasteur's fluid with $\frac{1}{5}$ % Na cl) the organisms had been starved and in a sense were too hungry to store food particles, but went to work at them immediately. There is no method conceivable by which such a supposition could be put to experimental test, for which reason it cannot be contradicted or proved.

The second supposition was that (2) absence of storage vacuoles might be caused by absence of bacteria, for in their normal environment the Protozoa are generally in close company with swarms of *Bacierium termo*, zooglea of micrococci and manifold spirilli and other schizomycetes, and by cultivation they had been brought into an almost aseptic, sterile environment.

The latter hypothesis is capable of experimental testing. For if bacteria will produce the phenomenon of storage then the supplying of septic food will be all that is requisite to add to the sterile solution. As a matter of fact it will be found that this is exactly what will happen. In a plasmodium that had shown 8 storage vacuoles in 24 hours of observation in a solution of $\frac{1}{2}$ % sodium chlorde (in distilled water) in which it had been kept one week, 48 storage vacuoles were observed in the next 10 hours on supplying dried albumen dust, moistened with the zoogloea from a Hay infusion.

Vorticellidæ which take in food particles readily are remarkably free from bacteria in their food vacuoles. Amoeba and plasmodia alike exercise to some extent a selective ingestion. Greenwood and Saunders claim to have watched *Amoeba proteus* for 14 days when surrounded with *Bact. termo*, vibrios and micrococci and the absence of bacteria from the endosarc was remarkable. They are taken in, it would seem, as unavoidable accompaniments of surrounding food only. Bacteria are not recorded to have been observed ingested by protozoa per se.

Another evidence of selective ingestion has been mentioned by Dantec, *l. c.*, as distinguishing between inert and living matter. Active monads or groups of spirilli are placed in marked vacuoles of ingestion, containing much of the acid secretion in comparison to inert matter which is usually invested very closely. We therefore have some evidence for assuming that plasmodia and Vorticellidæ distinguish between inert food and bacteria.

(1) Bacteria are rarely ingested except as unavoidable accompaniment of food. (2) Inert food, free from bacteria, is invested closely. Septic food within wide vacuoles. (3) In sterile environment, food in the stage of storage is the exception; in environment of bacteria, storage in acid vacuoles is frequent. I have brought these facts before you in this incomplete form, because the results are fairly uniform, and with the hope of stimulating further observation of the matter. These studies require no apparatus outside of the microscope and acid indicators. The general suggestion drawn from the result has a wider bearing than one would at first sight assume. For if further study will confirm that the ingestion of bacteria constantly prolongs the stage of maximum acidity from the usual time of 24 hours to several days in rhizopods. The suggestion is that the purpose of the acid is one of (disinfection) killing off bacteria.

There is a general uniformity of opinion that the presence of acid is unaccompanied by any digestive change on nutritive matter, which may be stored for many hours before it is dissolved and Greenwood and Saunders intimate that the endo-

sarc secretes some zymogen which perfects the digestive secretion.

The object to which the acid would seemingly serve in these organisms, which may be said to be on the very threshhold of life is the same which Bunge ascribes to it in man. Bunge's view is that the HCl has no other purpose than the sterilization of food. "Why should a chemical substance be placed in the entrance to the digestive tract," he asks, "in exactly the strength necessary for the destruction of bacteria which is directly antagonistic to the chemical reaction in which the main work of digestion must be carried on? The proteids are more readily converted into a solution lower down in the intestine and in an alkaline medium than by pepsin and acid. The object of the acid is, according to him, then, one of sterilization. This view cannot be denied, at the same time it must be admitted that HCl serves also a digestive purpose.

In the Rhizopods experimented upon, the observations of Greenwood and Saunders could be confirmed concerning the fact that while the acid is secreted in the food vacuoles under the stimulus of all ingesta; the true digestive vacuole which occurs only under the stimulus of nutritive matter apparently contains something besides an acid, perhaps an enzyme. The change in the acid indicators is as regards time and intensity of color transformation to all observation alike. There seems to be the same amount of acid in a storage vacuole as in a vacuole causing active solution of proteid matter, in close proximity to it, hence the assumption of an additional zymogenic substance in the latter is justifiable. As the amount of acid in one of these vacuoles is very small, and the change in Congo red to blue is speedy and striking, lends belief to the suggestion of Greenwood that the acid is an inorganic one. Why the protoplasm around a storage vacuole will not secrete zymogenic matter, though acid is clearly present in it, and at the same time this enzyme must be accepted to be present in a vacuole in which, close to the former, active digestion is going on is a question difficult to approach. If it can be demonstrated that all or most storage vacuoles contain some substance, living or inert, which is hostile to the economy of the Rhizopod and against which it protects itself by intensely acid

investment of the enemy for a prolonged period, a new and interesting light will be thrown on this phenomenon.

In the "Centralblatt für Bacteriologie, Parasitenkunde u. Infektions krankheiten, Vol. XIX, p. 785, Dr. C. Gorini describes a method for cultivating *Amoeba zymophila* on a solid medium which in this case is the potato. It is certain that Amoebae will grow on old and new potatoes with alkalinization. This would offer an easy and convenient method of cultivating them. It should be emphasized that it is almost impossible to produce cultures of amoeba that are absolutely free from bacteria. A. Celli in the Centralbl. f. Bacteriologie, Bd. XIX, p. 537, describes a number of futile attempts to obtain such cultures. For our purpose it is not essential that the amoebic cultures should be absolutely free from bacteria, a relative, approximate sterility is sufficient to demonstrate the scarcity of storage vacuoles in the amoebae and plasmodia in such environment. Celli's favorite solid medium is a preparation made from Fucus Crispus with 5 per cent Sterilized Water, with or without Bouilloñ, but always made alkaline. To 10 c.c. culture medium, 1 c. c. of an $\frac{N}{10}$ Solution of Potassium hydroxide, or 4–5 c. c. of a saturated solution of Sodium Bicarbonate. This culture medium of Fucus after it is made in the manner that Agar is generally prepared solidifies readily.

In the same Journal, Centrbl. für Bacteriologie, Band XIX, p. 258, Dr. M. W. Beyerinck describes a solid medium for amoebic cultures made from solidified agar by diffusion of the soluble organic substances in it into superimposed distilled water, which process requires about two weeks and repeated sterilization and subsequent addition of salts suitable to formation of nitrites.

I have no experience with these methods and have always found that for my purpose a solution of a little wheat bread in distilled water kept in a small flat dish under a glass cover was all that was required to have Amoeba and plasmodia of mycetozoa constantly on hand. The dish must be kept on a little earth and not in too bright a light and at a constant temperature. This simple culture medium, which of course is unsuitable for pure cultures was suggested by Prof. Reichert of the University of Pennsylvania.

THE BACTERIAL DISEASES OF PLANTS:

A CRITICAL REVIEW OF THE PRESENT STATE OF OUR KNOWLEDGE.

By Erwin F. Smith.

I.

It is scarcely fourteen years since Dr. Robert Hartig declared that there were no diseases of plants due to bacteria.[1] Two years later Dr. Anton de Bary, unquestionably one of the most learned and critical botanists the world has ever known and the foremost student of cryptogamic plants, expressed the belief that bacterial diseases of plants were of rare occurrence, and suggested as a partial explanation the fact that the tissues of plants generally have an acid reaction.[2] In his *Vorlesungen über Bacterien*, published in 1885, he expresses much the same opinion,[3] and cites only four diseases, viz., Wakker's hyacinth disease, Burrill's pear blight, Prillieux's rose red disease of wheat grains, and the wet rot of potatoes, described by Reinke and Berthold. Concerning the first of these four diseases he says: "Successful infection experiments and exact study of the life history of the bacterium are still wanting." Respecting the second he contents himself with briefly summarizing the statements made by Prof. Burrill. Of Prillieux's micrococcus he says: "Its importance as a cause of disease cannot be determined with any certainty from the brief account. It

[1] "Für die Krankheitsprocesse der Pflanzen kommen sie durchaus nicht in Frage, etc." Hartig: (*1*) *Lehrbuch der Baumkrankheiten*, 1882, p. 27.

[2] "Bacteria parasitic on plants have scarcely ever been observed, a fact to which R. Hartig has already drawn attention. One reason for this may be that the parts of plants have usually an acid reaction." De Bary: (*2*) *Vergleichende Morphologie und Biologie der Pilze Mycetozoen und Bacterien*, 1884, p. 520; English ed., p. 481.

[3] "According to the present state of our knowledge parasitic bacteria are of but little importance as the contagia of plant diseases. Most of the contagia of the numerous infectious diseases of plants belong to other animal and plant groups, principally, as already noted, to the true fungi." De Bary: (*3*) *Vorlesungen ueber Bacterien*, 1885, p. 136.

may turn out to be only secondary, appearing as a saprophyte in consequence of injuries previously received." Concerning the wet rot of potatoes he states that ordinarily it is a secondary phenomenon following the attacks of the parasitic fungus *Phytophthora infestans*, but admits that exceptionally potato tubers may become wet rotten without the presence of *Phytophthora*, and that "the above named observers succeeded in producing the appearance of wet rot in sound potato tubers by inoculations with their bacteria; in agreement with which stands a recent experiment of van Tieghem, who succeeded in totally destroying living potato tubers by means of *Bacillus amylobacter* when he introduced this into the interior of the tuber and maintained the same at a high temperature (35°)."

In the second edition of his *Lehrbuch*, published in 1889, Dr. Hartig modified his statements somewhat, expressing essentially the same opinions as de Bary. The yellow rot of hyacinths is recognized as a bacterial disease, although rather doubtfully in as much as it is said not to attack sound, well-ripened bulbs, under normal conditions, but only when they have received wounds or been attacked by fungi, especially by a hyphomycete which is said to be an almost constant accompaniment of the rot. The wet rot of potato tubers is admitted to the list, but with the statement that it is mostly a secondary matter, following the rot of stem and cells due to *Phytophthora infestans*. One other bacterial disease is mentioned, viz., pear and apple blight, with the suggestion, however, that it may have been erroneously attributed to bacteria, since the fungus *Nectria ditissima* produces in the bark numerous little bacteria-like gonidia.

Such was the general opinion on this subject down to within less than a decade. Even to-day the majority of well educated botanists would find nothing to contradict in the statement that there are very few diseases of plants distinctly attributable to bacteria. As a matter of fact, however, there are in all probability as many bacterial diseases of plants as of animals.

Various explanations have been advanced to account for this freedom or supposed freedom of plants from bacterial parasitism. As we have already seen, de Bary was inclined to ascribe

it in good part to the acid reaction of vegetable tissue. Dr. Hartig's view is best expressed in his own words:[4] "Whereas the processes of decay, and most of the infectious diseases of man and animals, may be traced to bacteria, the plant organism is protected against them by the peculiarity of its structure, and especially by the absence of circulatory channels for conducting the nutrient fluids which could serve to distribute any lowly organisms which might happen to be present in the food. It is only by means of the vessels and intercellular spaces that they can distribute themselves in any great numbers in the body of the plant, for in other cases they have to pass through the cellulose or woody cell walls, which offer great resistance to their attack. In addition to this, the vegetable juices, most of which show an acid reaction, are unfavorable to their growth. As a matter of fact, bacteria have hitherto been found only in the tissues of plants whose cells are parenchymatous in character and possessed of very delicate walls, as for instance, bulbs and tubers."

For several years Ph. van Tieghem experimented with one or more, probably several, bacteria, called by him *Bacillus Amylobacter* and believed to be the specific agent in the decomposition of cellulose. In 1879,[5] he stated that all the cells of all plants are equally dissolved by it in the meristematic stage but that as soon as the tissues have become differentiated profound differences are noticeable. The cellulose of many plants is dissolved by it but that of mosses, sphagnums, hepatics, lycopods, fern leaves, and stems and leaves of phanerogamous aquatics proved resistant. This behaviour of water plants is "une nécessité d'existence." In 1884,[6] he made a number of additional similar statements. The tubers of the potato, the seeds of beans (first swelled in water and then inoculated directly into the substance of the cotyledons), and the fruits of cucumbers and melons rotted quickly when infected with this organism. Inoculated leaves of Crassulaceæ and stems of Cac-

[4] Hartig: *Lehrbuch*. 2nd. Edition. English translation, p. 37.

[5] Van Tieghem: (*4*) Sur la Fermentation de la Cellulose. *Bull. de la Soc. Bot. de France*, 1879, pp. 25 to 30.

[6] Van Tieghem: (*5*) Développement de l'Amylobacter dans les plantes à l'état de vie normale. *Ibid.*, 1884, pp. 283–287.

taceæ resisted until plunged under oil when they decayed quickly. Aquatics resisted: "By means of a Pravaz syringe I have injected a drop full of the spores of Amylobacter into the lacunary system of several submerged aquatics (Vallisneria, Helodea, Ceratophyllum) but always without result. The plant remained healthy in all its parts."

These papers of van Tieghem are often cited, but they have little substantial value. Undoubtedly he believed that he was experimenting with pure cultures, or, at least, that the results obtained were due to *Bacillus amylobacter*, but such is, to say the least, very improbable. *B. amylobacter* is now believed to be strictly anaerobic, and incapable of any action on cellulose.[7]

More recently Julius Wiesner has divided all plants into two classes, ombrophobic and ombrophylic plants, according as they are or are not readily injured by prolonged rains or exposure to stagnant fluids.[8] His experiments show that the aerial parts of some plants rotted very quickly when exposed to continuous artificial spray while similar parts of other plants proved very resistant, remaining sound for weeks (62 days in case of *Tradescantia guianensis*). The same contrast was observed when leaves of the two sorts of plants were placed in stagnant water, the former lost their turgor and rotted in a few days, the latter proved much more resistant. Many land plants have this power of resistance and all water plants, also all underground parts, even the roots of plants having very susceptible foliage. As additional confirmation Wiesner states that when meat infusions are left to themselves they always decay much sooner than when fragments of ombrophylic plants are placed therein. Ombrophobic plants in water or meat infusion also decay less rapidly when mixed with fragments of ombrophylic plants than when left to themselves. This decay is more rapid in the dark than in light, especially

[7] Prazmowski: (*6*) *Untersuchungen ueber die Entwickelungsgeschichte und Fermentwirkung einiger Bacterien-Arten.* Leipzig, 1880, pp. 23–37.

[8] Wiesner: (*7*) Ueber ombrophile und ombrophobe Pflanzenorgane, *Sitzungsb. K. Ak. d. Wissenschaften*, Math.—Naturw. Classe. Wien., 1893, Bd. 102. Abt. I, pp. 503–521. See also Wiesner: (*8*) Pflanzenphysiologische Mittheilung aus Buitenzorg (III). Ueber den vorherrschend ombrophilen charakter des Laubes der Tropengewächse. *Ibid.*, 1894, Bd., 103, pp. 169–191.

bright light. The foliage of ombrophylic plants is easily wetted; that of ombrophobic plants is as a rule not readily wetted, being usually protected by bloom or some other device for warding off water. When ombrophobic plants are not protected in some such manner, decay is remarkably rapid. In general if the leaves of a plant are readily wetted, it may be assumed that they are ombrophylic, but there are exceptions, e. g. the potato and tomato. Roots of all plants are extraordinarily resistant. In most plants middle aged leaves are least susceptible to decay but in the potato the youngest leaves resist best. Old leaves lose this power of resistance. Sometimes this resisting power is variable in different individuals of the same species, depending on the conditions under which they have been grown. Curiously, all plants of shady, damp places are ombrophobic, if they possess leaves which are not readily wetted, e. g. Impatiens. The more the parts of a leaf are divided the quicker the decay. The green parts of the following plants are mentioned as particularly susceptible to bacterial decay: *Solanum tuberosum*, *Lycopersicum esculentum*, *Xeranthemum annuum*, *Impatiens nolitangere*, *Chenopodium album*, *Veronica burbaumii*, *Viola arvensis*, and *Taraxacum officinale* (from sunny, dryplaces) *Mimosa pudica*, *Pisonia alba*. The following plants were found to be very resistant: *Ranunculus aquatilis*, *Lemna minor*, *Lysimachia nummularia*, *Begonia magnifica*, *Tradescantia zebrina*, *T. guianensis*, *Selaginella sp.* (from green house), and *Scolopendrium officinarum*. Among underground organs the roots of the yellow beet proved most resistant. The author's general conclusion from these experiments is best expressed in his own words: "It can now be stated as highly probable that the power of ombrophilous organs to resist rain for months is referable chiefly to the fact that antiseptic substances are produced in the tissues of the organs." These experiments are interesting but seem to have been performed in a rather crude way. The relative rapidity of decay was determined by appearance and the sense of smell and the organisms inducing this decay were undetermined. These experiments should be repeated and extended by Dr. Wiesner

or by some bacteriologist, using pure cultures and plant juices which have been sterilized by filtration.

Dr. Russell's experiments[9] were made a year earlier than than those of Wiesner and have the merit of being properly performed, i. e. with sterile juices and pure cultures so that the conditions under which the experiments were made can be reproduced by other investigators. They are, however, too limited in number to afford any basis for a general conclusion. He found that Canna juice, sterilized by filtration, exerted no appreciable germicidal effect on any of the following species: Kiel-water bacillus, *B. lactis-ærogenes*, *B. coli-communis*, *B. megaterium*, *B. prodigiosus*. Experiments with *B. megaterium*, *B. butyricus*, *B. coli-communis*, *B. pyocyaneus*, and *Streptococcus pyogenes*, using as a culture medium root-pressure juice obtained under sterile conditions from the severed stem of lima beans and Pelargoniums led to a similar conclusion and to the enunciation of the following general statement: "vegetable cell juices, aside from their acid reaction, are entirely powerless against bacteria, and do not possess any germicidal properties like the blood serum of animals."

The old view that plants are not subject to the attacks of bacteria simply because their tissues are acid was shaken by the discovery that some bacteria grow very well in acid media, and was thoroughly upset by the discovery that the juices of some parts of many plants are alkaline. In all probability plants like animals require a delicate balance between acid and alkaline and a continual change from one side to the other for the carrying on of the life processes. Three things at least are certain (1) It will not do to assume that all parts of a plant are acid because some part of the parenchyma shows a strongly acid reaction; (2) It cannot be stated that any given microorganism will thrive only in alkaline media until this fact has been determined by direct experiment; and (3) Many bacteria, perhaps all, are alkali producers and capable, if they can gain any foothold whatever, of slowly changing an unsuitable acid medium into one more alkaline and better adapted to their use.

[9] Russell: (*9*) *Bacteria in their Relation to Vegetable Tissue.* Thesis. Johns Hopkins University. 1892, 8vo. p. 41.

Wiesner's hypothesis is somewhat different. It has been known for some time that various essential oils and other vegetable products, e. g. thymol, salicylic acid, benzoic acid, tannin, quinine, oil of cinnamon, oil of peppermint, etc., exert a powerful restraining influence on the growth of many bacteria, and it is not improbable that a great variety of bactericidal and protective substances occur in plants. On the other hand there may be and probably are bacterial parasites capable of thriving in the very plants which Wiesner found most resistent to continuous spray, to the saprophytic bacteria of stagnant water, and to those of decaying meat infusions, the exact conditions under which any given microorganism will thrive being determinable only by experiment. It must also be remembered that the physiological requirements of bacteria often become profoundly modified to suit changed environments, and that all parasites have undoubtedly descended from saprophytic forms. Prof. Wiesner has, however, opened up a very inviting field and its further investigation by some careful experimenter, trained in bacteriological methods, might lead to very interesting discoveries.

Most of the recent books on vegetable pathology devote a chapter to the bacterial diseases of plants, but these books have not been written by bacteriologists and consequently the statements given are usually very meager and unsatisfactory, and forcibly illustrate the fact that no one can write acceptably on a subject with which he is not familiar, not even if he possesses a logical mind and has read all the "authorities." Excepting Prof. W. Migula, who reviewed the subject briefly but somewhat carefully in 1892,[10] and Dr. H. L. Russell, who gave a brief summary in tabular form the same year at the end of his Thesis,[11] no one seems to have gone over the field critically since de Bary's time, although there is now a considerable body of literature. It is proposed, therefore, in the following pages to examine the literature of this subject from the standpoint of the

[10] Migula: (***10***) *Kritische Uebersicht derjenigen Planzenkrankheiten, welche angeblich durch Baktrien verursacht werden.* Semarang. Midden-Java. 1892. Exp. Sta.

[11] Russel: l. c., pp. 36–41.

modern bacteriologist, sifting as far as possible the wheat from the chaff, and arranging all in an orderly way for convenient reference. The utility of such a piece of work, if well done, can scarcely be questioned, since it must set into sharp relief the gaps in our knowledge and tend to stimulate further research.

The work of the early investigators already mentioned was done before the perfection of modern methods of bacteriological research, and in a time of general scepticism which some of us well remember. It is therefore in no way discreditable that many of their conclusions should be found untenable when tested by the more rigid requirements of the science of to-day. They worked under great difficulties and did as well as could be expected even of men of genius, better, indeed, than many of us would have done. Certainly, as pioneers in a difficult field they deserve great credit.

As much cannot be said for some of the more recent workers who with every opportunity in the way of literature, including numerous manuals of bacteriology, and with laboratory facilities for learning the fundamentals of bacteriological research on every hand in every land, have been content to publish second and third class work, exactly like that preceeding the discoveries of Pasteur and Koch and the development of modern methods. One might suppose these people to have been in a deep sleep for the last twenty years, they take so little note of what has been going on. I shall have frequent occasion to consider papers of this class in the course of these pages and shall not fail to point out their worthlessness, to discourage imitators, if for no other reason. It goes without saying that such publications do not advance science, nor in the end in any way contribute to the reputation of the individual. They are thoroughly discreditable, and in case new species are erected, are little less than criminal, considering the present overburdened and chaotic state of systematic bacteriology.

Thanks to the itch for species making, systematic mycology is generally cited as the most desperately confused and perplexing branch of natural science, but mycology is a highway turnpiked and provided with arc lights in comparison with the wilderness of systematic bacteriology. Of the thousand or

more forms which have been studied and named, or designated by letters or figures or vernacular names,[12] probably not one-tenth can be identified with any certainty owing to the meagerness of the descriptions. The older descriptions are particularly bad, and the effort to decide what was meant by these old names, for which somebody will by and by be strenuously claiming inalienable rights of priority, is usually time thrown away. There is quite enough to do in bacteriology, as every one knows who is dealing at first hand with its hard problems, without wasting precious energy in striving to guess what was meant by two and three line descriptions. All descriptions which do not *describe*, and there are many such, ought to be wholly ignored, and no species recognized as worthy of a place in literature unless so characterized as to be identifiable by others. A plea of this sort in the higher branches of botany or zoology would be a subject for laughter. Bad descriptions are however, so much the rule in bacteriology that it is no laughing matter but rather a great evil urgently demanding reform. It is a state of affairs which has come about naturally enough considering the way in which bacteriology has developed[13] but which would not now be tolerated for a moment in phanerogamic botany or in most branches of zoology and the continuance of which in bacteriology it is incumbent on every honest worker to limit and discourage in all possible ways. The best way in science, always, is to speak out plainly, and to join hands for the advancement of a good cause. Bad work should be ignored and "new species" relegated to limbo unless the descriptions conform to the requirements of modern bacteriological science, meaning by this expression the consensus of opinion among experienced and careful investigators everywhere. If there were some such agreement among the better class of workers, the improvement in systematic bacteriology would become very marked. The volume of publication would, indeed, decrease noticeably but this of itself

[12] About 650 species are mentioned in (***11***) Schizomycetaceæ, by de Toni and Trevisan in Saccardo's, *Sylloge Fungorum*, VIII, published in 1889, but this is not complete.

[13] All the early systematists built upon a foundation of sand, i. e. upon pure morphology.

would be a great advantage, and the quality of the work would more than correspondingly improve. Only in some such way can the strong tendency toward trashy publication be eliminated and light and order evolved from the present chaos.

With few exceptions, vegetable pathology seems to have been specially unfortunate in the class of persons who have devoted themselves to the study of bacterial diseases. While the bacterial side of animal pathology has had its Pasteur and Koch, its Esmarch, Hueppe, Flügge, Gaffky, Frænkel, Pfeiffer, Lœffler, Duclaux, Metchnikoff, Chamberland, Roux, Welch, Sternberg, Smith, Prudden, and a host of other skilled experimenters, scarcely less eminent, and has made correspondingly great progress, the study of the bacterial diseases of plants has been principally in the hands of botanists without special laboratory training in bacteriology and even destitute in some cases of an elementary knowledge of right methods of work. The great development of modern bacteriology is attributable largely to the discovery that human diseases are due to these organisms, and to its consequent alliance with medicine, but there is no reason why the same rigid scrutiny of methods and sharp calling in question of statements which have led to such brilliant results in animal pathology in recent years should not be applied in the same way to vegetable pathology. Accurate experimentation and trustworthy results are from a purely scientific standpoint quite as desirable in one field as in the other.

Two things are especially to be kept in mind in describing any bacterial disease of plants: (1) The pathogenesis must be worked out conclusively; (2) If the organism is named, it must be so described that it can be identified by any competent bacteriologist no matter where it is found.

The four requirements under the first head, i. e. *Pathogenesis*, are now generally recognized to be as follows:

A. Constant association of the germ with the disease.

B. Isolation of the germ from the diseased tissues and study of the same in pure cultures on various media.

C. Production of the characteristic symptoms of the disease by inoculations from pure cultures.

D. Discovery of germs in the inoculated, diseased tissues, re-isolation of the same, and growth on various media until it is determined beyond doubt that they are identical with the organism which was inoculated.[14]

Not one of these steps can be omitted. Possible sources of error beset the investigator at every step, and anything short of a rigid demonstration cannot be accepted as proof. A. is usually quite easy, involving only the careful microscopic examination of abundant material, stained and unstained. B. was made possible by the improvement of methods, *i. e.* by the use of solid media, and especially by the discovery of the method of isolation by means of plate cultures. C. is quite easy, provided the right organism has been obtained and this be inserted into the proper tissues under the right conditions to insure growth. The fulfillment, however, of these conditions often involves long and vexatious delays, and taxes the acumen of the investigator to the utmost. D. serves as a check on all the preceding work, showing that there has been no unintentional mixing of organisms, and that the results obtained were actually due to the supposed cause. For the sake of brevity these four rules of practice will be referred to in the following pages simply as A. B. C. and D. What weight shall be given any specific statement depends of course on the reputation of the individual. Some men are so careful of their reputation and so little given to making unwarranted statements that their simple word goes a long way even when the statements themselves seem improbable, whereas the elaborate explanations of other men, if the asserted facts are at all out of the ordinary, have to be taken with a grain of salt.

The requirements under the second head, i. e. *Description of the organism*, are more numerous, and are embraced under two general divisions of very unequal value, namely *Morphology* and *Biology*. In the classification of the higher plants and animals morphology has been accepted from time immemorial

[14] A series of successful reinoculations is always very desirable and becomes indispensable in case the infections are obtained on plants grown in a locality where the disease prevails naturally. Of course, numerous un-inoculated plants, known as "checks" or "controls," must always be reserved for comparison.

as answering all the requirements of systematists, but such is far from being the case when it comes to the description of bacteria. These minute organisms, which are among the lowest and simplest forms of living things yet discovered by man, are, within the commonly accepted generic limits, so morphologically similar as very often to be indistinguishable with any certainty even under the highest powers of the microscope. As supplemental, therefore, to morphology, and even in many cases as a complete substitute for it, we must have recourse to *Biology*, viz. to the behaviour of the living organism under a variety of known, artificially prepared conditions, such for example as the peculiarity of its growth on various culture media, its thermal death point, its ability to ferment various sugars, the chemical products of its growth, its pathogenic power, etc. Morphologically identical organisms often differ so widely and constantly in their biological peculiarities that there can be no question about their being distinct species, or as to the real value of this means of classification. Probably it also has value, hitherto overlooked, for the differentiation of higher plants and animals, especially for determining the limits of polymorphic or closely related species.

It is not my intention in this place to mention all the biological tests which should be applied to any species for its proper characterization. These are being added to constantly by an army of trained workers in all parts of the world, and my own views of what is at present necessary, or at least highly desirable, will be sufficiently evident in what is to follow. Very likely, also, as knowledge increases, some of the tests which are now generally held to be important will be shown to have little specific worth.

This, however, appears to be a good place to insist on accuracy in all the details of bacteriological work, especially in the preparation of culture media, and on explicitness of statement so that other investigators may know just what was done and how it was done, and thus be able to repeat the experiment. When all details of work are suppressed the inference, naturally enough, is that the writer was ignorant or else that he desired to conceal something not specially to his credit, and which if

plainly expressed might millitate against the value of his work. Either horn of the dilemma is equally bad. Some, however, who are desirous of doing good work in this field, or at least appear to be conscientious workers in other lines, do not seem to be aware of the necessity for extreme care in the preparation of culture media and the management of cultures. As a matter of fact, many bacteria are extremely sensitive to slight changes in the composition of the media in which they are grown or to other conditions within the control of the experimenter, and this appears to be true especially of the pathogenic species. Hence the many conflicting statements about the same organism. A few examples will render my meaning plainer. The careless exposure of cultures to bright sunshine may destroy the organism. An organism supposed to come from diseased tissues or from a culture, and which is being examined in a cover glass preparation, may have been derived actually from a contaminated staining fluid. The apparently simple matter of slightly unclean test tubes or flasks may lead to error, e. g. antiseptic influences may be at work, or preciptates may be thrown down and subsequently mistaken for bacterial growth. Some kinds of glass are unsuited to delicate bacteriological work, the culture fluids being contaminated by substances dissolved out of the walls of the beakers, tubes, and flasks. Tyros, of course, are liable to mistake almost anything for bacteria or to find them anywhere (See a long paper by Bernheim on (*12*) Die parasitären Bacterien der Cerealen, in *Münch. med. Wochenschrift*, 1888, pp. 743–745 and 767–770, and comments on the same by Buchner and Lehmann, *Ibid.*, 1888, p. 906, and 1889, p. 110). Boiling culture media, after it has been compounded, in open beakers or in loosely plugged test tubes or flasks may unwittingly lead to its concentration. The use at different times of different peptones, or grades of gelatine, of unlike per cents of gelatine or agar, of varying grades of acidity or alkalinity, of impure chemicals, of different concentrations of the nutrient media, and of different methods in its preparation all tend to render comparative studies impossible. A large source of error in the differentiation of species by means of sugar fermentation experiments has been the employment of bouillon

containing undetected muscle sugar. Even when preliminary tests are made with some gas-producing bacillus there is still an opportunity for error, provided the tests are carried on only for a day or two. No bouillon should be judged free from sugar and safe for use until in fermentation tubes it has been subjected for at least a week to the influence of *Bacillus cloacæ* or some other organism producing an abundance of gas from grape sugar. If at the end of this period no gas has developed, and the transfer of a loop of fluid from such a tube into another fermentation tube containing a dextrose-bouillon sets up an evolution of gas, then the first bouillon may be used with confidence. Again, if cane sugar is sterilized in an acid bouillon at least a part of it is *inverted*, i. e. changed into dextrose and fructose, and fermentation results obtained therefrom may be due to the presence of any one of three sugars. Bouillon should always be made distinctly alkaline before cane sugar is added. Many of the older fermentation experiments are worthless on account of neglect of such precautions, to say nothing of some recent ones. Again *Bacillus tracheiphilus* grows not at all or feebly on nutrient gelatine as ordinarily made, or in media which is acid beyond a determinable slight degree, and if only such media were used the erroneous conclusion might be reached that it could not be grown outside of the host plant, whereas it grows freely in artificial media, even on gelatine, when the right conditions are established. *Bacillus amylovorus* grows well in some gelatines and refuses to grow in others. Even comparatively slight changes in the acidity or alkalinity of the culture media often have a marked effect on the growth of certain organisms, while others, *e. g.*, *Bacillus cloacae*, are able to grow in almost any medium. Many bacteria prefer alkaline media, and some are very sensitive to the presence of acids, while a variety of bacteria commonly met with in water will not develop at all if the medium is rendered strongly alkaline. Other organisms grow well in acid media.[14a]

[14a] For a striking illustration of the effect on the growth of water bacteria of comparatively slight charges in the reaction of gelatine, see a recent table by George W. Fuller, in a paper entitled: (*13*) On the proper reaction of nutrient media for bacterial cultivation.—*Journal of the American Public Health Association*, Concord, N. H., Oct., 1895, p. 393.

Even the slightly varying acidity of steamed slices from different potato tubers may exert a marked effect on the growth of certain sensitive organisms. On this account some bacteriologists have advised discarding the potato altogether. I have myself found the potato a very useful substratum for the growth of both fungi and bacteria. All comparative tests on potato ought, however, to be made on cylinders or slices cut from the same tuber, and in every case the reaction, acid, neutral, or alkaline, should be carefully recorded. The behavior of the organism on a variety of tubers should also be determined, before deciding that it is something new. It has been thought by some that the best nutrient substance for a parasite must be, unquestionably, the juices of the host plant but this does not follow since there are all grades of parasitism, and even if it did, there are several chances for error in its employment, e. g. the nutrient juices are usually sterilized by steam heat and this may cause a number of chemical changes resulting in a compound very different from the living plant and entirely unsatisfactory as a culture medium, as many have learned by experience. Again, for some particular reason, even the juices of the plant when sterilized at ordinary temperatures by filtration, may be less well adapted to the needs of the parasite than well made beef bouillon or ordinary nutrient agar. In general, the standard culture media of bacteriology should be tried first. Some bacteria can be cultivated only on special media or at special temperatures, or under unusual conditions. *Bacillus subtilis* will only grow in the presence of free oxygen; *Bacillus amylobacter*, *B. tetani*, and *B. carbonis* will only grow in the absence of oxygen. Winogradsky states that his nitrifying organism obtained from European soils will not grow in the ordinary culture media and thrives best in solutions of inorganic substances, and on silicate jelly. *Bacterium tuberculosis* can be cultivated only in bouillon and on blood serum and nutrient glycerine agar, and at temperatures above 30°C. *Bacterium influenzæ* also flourishes at blood heat and can only be grown, it is said, in the presence of red blood corpuscles or in media containing yolk of eggs; other organisms have thus far refused to be cultivated at any temperature or on any artificial medium, e. g. *Bacterium lepræ* and *B. syphilitis.* Some bacteria

are destroyed at temperatures at which careless workers frequently pour their agar plates, while others refuse to grow at ordinary temperatures or even at blood heat, grow best at 50°--60°C., and are not killed until the temperature exceeds 70° or even 75°C. Finally, a race of *Bacterium anthracis* incapable of producing spores has been developed by growing the organism in media containing phenol; another non-virulent race bearing swollen, terminal spores, "drumsticks," by growing the organism in compressed air; and still another race destitute of virulence by cultivating it at temperatures above 40°C. These are not exceptional cases, similar care being necessary in all directions if one would avoid erroneous conclusions.

Naturally, every successful experimenter will vary his culture media in all sorts of ways in order to learn as much as possible of the organism under consideration, but at the same time he will determine its behaviour on the standard media, and will keep a very careful record of all that he does. The bacteriologist should make it an invariable rule to repeat every experiment two or three times, at the very least, and generally after an interval of some months or years he should repeat all his experiments. Even then he will fall into errors enough. He certainly should proceed with as much care as the chemist, and in many directions the work passes naturally over into chemistry. If quantitative or volumetric analysis requires all sorts of precautions and excess of care to avoid errors, no less does this youngest of all the sciences.

A few words respecting the occurrence of bacteria in normal plant tissues will be in place before concluding these general remarks. It goes without saying that such minute and universally distributed bodies as bacteria are likely to be found at times almost anywhere, even in plant tissues which seem to be healthy, just as they may sometimes occur in the blood stream of healthy animals, but they are not normally present in the tissues of plants. All carefully conducted experiments have led to this conclusion. The reader who wishes fuller information may consult papers by Laurent,[14b] Buchner,[15] Lehmann,[16]

[14b] (***14***) Sur la pretendue origine bacterienne de la diastase. *Bull. de l'Acad. roy. de Belgique*, T. X., pp. 38–57.

[15] (***15***) Notiz betreffend die Frage des Vorkommens von Bacterien in normalen Pflanzengewebe. *Muench med. Wochenschrift.*, 1888, pp. 906–907.

[16] (***16***) Erklarung in Betreff der Arbeit von Herrn Dr. Hugo Bernheim, etc. *Ibid*, 1889, p. 110.

Fernbach[17] Vestea,[18] Kramer,[19] and Russell.[20] Even when purposely introduced into living tissues they refuse to grow or spread but little and finally die out,[21] unless they possess specific pathogenic power in which case the result is quite different.

The diseases which will be discussed in the following pages may be divided into four classes:

(1). Diseases of clearly established bacterial origin.

(2). Diseases which appear to be constantly associated with bacteria and which are probably due to some specific organism, but full proof of which has not been furnished.

(3). Diseases said to be more or less closely associated with the presence of bacteria and ascribed thereto, but in which little or no proof has been brought forward to establish the causal relation.

(4). Communicable diseases which have been ascribed to bacteria but associated with which no organism has been found and which are probably of non-bacterial nature.

On the whole it would perhaps be more logical to divide the following pages into four chapters in the way I have specified, but for practical reasons it has seemed better to discuss all of the diseases of a given plant in one place. I have, therefore, arranged the material by hosts, but will at the close try to summarize the whole subject in the manner above indicated.

It will certainly be some time, probably many years, before we have anything like a permanent scheme of classification for the bacteria. Our knowledge is still too incomplete. Meanwhile, we have to do the best we can with the present systems, all of

[17] (*17*) De l'absence des microbes dans les tissus vegetaux. *Annales de l' Inst. Pasteur*, 1888, pp. 567–570.

[18] (*18*) De l'absence des microbes dans les tissus. *Ibid.*, 1888, p. 670–671.

[19] (*19*) Bakteriologische Untersuchungen ueber die Nassfäule der Kartoffelknollen. *Osterreichisches landw. Centralb.* I, Heft 1, 1891.

[20] l. c.

[21] Lominsky: (*20*) On the parasitism when introduced into plants of some disease-causing microbes (Russian). Wratch., 1890. No. 6, pp. 133–135.

Russell: l. c.

Kornauth: (*21*) Ueber das Verhalten pathogener Bakterien in lebenden Pflanzengeweben. *Centrb. f. Bakt., Parasiten-Kunde, u. Infectionsk.* I Abt., Bd. XIX, No. 21, 8 Juni, 1896, pp. 801–805.

which are more or less arbitrary and unsatisfactory, and all of which are liable to be set aside at any time. I have here adopted Migula's system[22] which seems to me very convenient, and on the whole the most satisfactory of any that has yet appeared.

Before proceeding to the body of this review it only remains to say that every effort has been made to deal impartially with the material in hand, and to present the essential ideas of the writers as concisely and accurately as possible. To this end the original papers have been consulted in every instance, unless otherwise stated in the text. So much vexation over wrong references has been experienced in time past by the writer that he has himself been at special pains to give full and accurate citations. It is to be hoped, therefore, that the reader will have no difficulty in finding the original papers. An endeavor has also been made to bring the subject fully up to date but it is quite likely that some worthy papers may have been overlooked, owing to the many languages and the ever increasing number of places of publication.

THE MEANING AND STRUCTURE OF THE SO-CALLED "MUSHROOM BODIES" OF THE HEXAPOD BRAIN.

By F. C. Kenyon, Ph. D.[1]

In looking at a series of sections of the brain of a hexapod, especially of a hymenopteron, the most notable structures are two pairs, one to each side, of large cup-shaped bodies of "Punkt substanz," or, what in the light of our present knowledge of nerve structure is better denominated fibrillar substance. Each of these cups is filled to overflowing with cells having large nuclei and very little cytoplasm. From the under surface

[22] Migula: Schizophyta: (*22*) Schizomycetes. *Die Natuerlichen Pflanzenfamilien* (Engler u. Prantl). I Teil. 1 Abt. a, Lief. 129 8vo. p. 44, Leipzig, 1896. This is the forerunner of a larger work soon to be published by Gustav Fischer, Jena.

[1] Clark University, Mass.

each of these cups or "Becher" there descends into the general fibrillar substance of the brain a column of fibrillar substance which unites with its fellow of the same side to send a large branch obliquely downwards to the median line of the brain and an equally large or larger branch straight forwards to the anterior cerebellar surface. (Fig. B.)

Long before our present methods of sectioning and staining had found general application in the study of animal structure, or as early as 1850, the French naturalist, Dujardin, discovered these bodies in transparent preparations *in toto* of the brains of certain Hymenoptera and Orthoptera. From their somewhat folded appearance he describes them as "lobes à convolutiones," and compared them with the convolutions of the human brain, and even thought them associated with hexapod intelligence. Fourteen years later, Leydig, using the same methods confirmed Dujardin's discovery in working with the brains of the ant, bee, and wasp, and described them as "gestielter Körper." In 1875 Rabl-Rückhard identified the bodies in *Gryllus italicus*, *Locusta viridissima*, and *Dycticus verrucornis*, and correctly described the form of the "cup" under the term "Rind Körper." The very next year ('76) Dietl's application of the section method to the subject confirmed and perfected previous descriptions, and, struck with the resemblance to mushrooms, he adopted the name of "Pilzhutförmiger Körper," a conception later used by Packard (mushroom bodies) and by Bellocici ('82) (corpo fungiformo).

As to the intellectual function of the bodies, not all of the early writers supported Dujardin's inference. They were supposed to be connected with sight; but Rabl-Rückhard showed that they are fully developed in a blind African ant (Typhlopone). Dietl was loth to acknowledge an intellectual function, even though he found the organs more highly developed in Hymenoptera than in Orthoptera. But Forel ('74) adhered to Dujardin's supposition, and showed that among Hymenoptera even of the same species the bodies are most prominent where one usually recognizes most intelligence, as in the worker bees and ants, while they are small in the females and the males. Brandt ('76) two years later in a note on the brain of Hymen-

optera makes the same observations as to the differences in the same species, while Berger ('78) considered the structures as "organs of projection of the first order."

The supposition of Dujardin obtained its best support so far as the older methods would avail in the comprehensive work of Flögel ('78) covering the whole group of hexapods. Here, one may see at a glance that the development of the structures largely coincides with the development of intelligence, as shown by the following abridgement of his table:

A. The four cups completely developed.	
1. Very highly developed,	*Vespa.*
2. Large with rim,	*Apis, Formica Pompilus, Ichneumonidæ.*
3. Without rim,	*Blatta.*
4. Very small,	*Cossus, Sphinx, Vanessa.*
B. Cups incomplete.	
5. Walls and cells so reduced as hardly to be recognized as cups,	*Tenthredo, Cynips.*
6. Reduced to two small heaps,	Many small butterflies.
7. Wall a broad plate,	*Forficula, Acridium,* etc.
8. Wall (fibrillar substance) absent.	
(*a*) Cells in 4 groups,	*Dycticus.*
(*b*) Cells in 2 groups, distinguishable by comparison with neighboring cells,	*Aeschna.*
(*c*) Not so distinguishable,	*Tabanus.*
C. Cups unrecognizable even as rudiments,	*Hemiptera.*

If such a superior neural function is indicated by the testimony and work of the earlier writers, it may well be asked whether recent neurological methods will bring out the structure of the hexapod brain as well as they have that of the other invertebrates and that of the vertebrates, and whether they will lend this view support. First, it may be noted that the physiological experiments of Binet ('94), which are those of

Faivre very much bettered, demonstrate that a hexapod may live for months without a brain, if the subæsophageal ganglion, or better, ventro-cerebron, is left intact, just as a vertebrate may live without its cerebrum. Faivre long ago showed that this ventro-cerebron is the seat of the power of co-ordination of the muscular movements of the body. Binet has shown that the brain is the seat of the power directing these movements. A de-brainedhexapod will eat when food is placed beneath its palpi, but it cannot go to its food even though the latter be but a very small space removed from its course or position. Whether the insect would be able to do so if the mushroom bodies only were destroyed, and the antennal lobes, optic lobes, and the rest of the brain were left intact, is a question that yet remains to be answered. In Binet's experiments neither olfactory nor visual stimuli can be transformed into motor impulses. Were it possible for them to be so transformed, my studies to be noted in a moment cause me to think that Binet's results would be very materially altered.

Now, as to my studies. During the winter just past with no little patience I endeavored to apply the bichromate of silver method to a study of the brain and general nervous system of the common honey bee, the more detailed result of a portion of which will be published a little later. The endeavor was rewarded by a considerable degree of success, the main facts being determined, though there are many details left for future studies. Others have tried to employ the same general method, but owing to a lack of proper store of patience or to their setting about the task wrongly have failed. Among them must be counted Binet ('94), with whom, however, there seems to be a defect in the conception of both the Golgi and the Erlich methods. For he sets the former aside as inconstant, uses the latter, without, however, apparently obtaining any very good results. He complains that preparations by the Erlich method (and the Golgi method might be included) leave out many details, and never seems to think that a sufficient number of preparations will supply those details and thus allow the whole to be determined. This is the more unfortunate, since his dependence upon the old methods has led him to give detailed

importance to phenomena that are relatively unimportant, and has resulted in a somewhat misty conception of the structure of the hexapod ventral nervous system.

One of the very first things that an impregnation of bee brains with bichromate of silver enabled me to make out was the structure of the mushroom bodies with their cells. These cells stand out in sharp contrast to all other nerve cells known, though they recall to some extent the cells of Purkinje in the higher mammals. Each of the cells contained within the fibrillar cup seeds a nerve process into the later, where it breaks up into a profusely arborescent system of brahchlets, which often appear with fine, short, lateral processes, such as are characteristic of the dendrites of some mammalian nerve cells. Just before entering the fibrillar substance a fine branch is given off that travels along the inner surface of the cup along with others of the same nature, forming a small bundle to the stalk of the mushroom body, down which it continues until it reaches the origin of the anterior and the inner roots mentioned at the beginning of the paper. Here it branches, one branch continuing straight on to the end of the anterior root, while the other passes to the end of the inner root. Throughout its whole course the fiber and its two branches are very fine. Nearly the whole stalk and nearly the whole of each root is made up of these straight parallel fibers coming from the cells within the cup of the mushroom bodies. What other fibers there are enter these bodies from the side, and branch between the straight fibers very much as the dendrites of the cells of Purkinje branch among the parallel fine fibers from the cells of the granular layer in the mammalian cerebellum. These fibers are of the nature of association fibers.

From the olfactory or antennal lobe, from the optic ganglia there are tracts of fibers that finally enter the cups of the mushroom bodies as shown by Viallanes and by my studies with the Golgi method and also with a Formol-copper-hæmatoxylin method of staining. Besides these tracts the Golgi method has enabled me to make out another tract, unknown before, passing down the hinder side of the brain from the cups to the region above the œsophagus, where it bends forwards and comes in

contact with fibers from the ventral cord, which exists, although Binet was unable to discover any "growth of fibers connecting the cord with the brain."

The fibers entering the cups from the antennal lobe, the optic ganglia, and the ventral region, spread out and branch among the arborescent endings of the mushroom body cells.

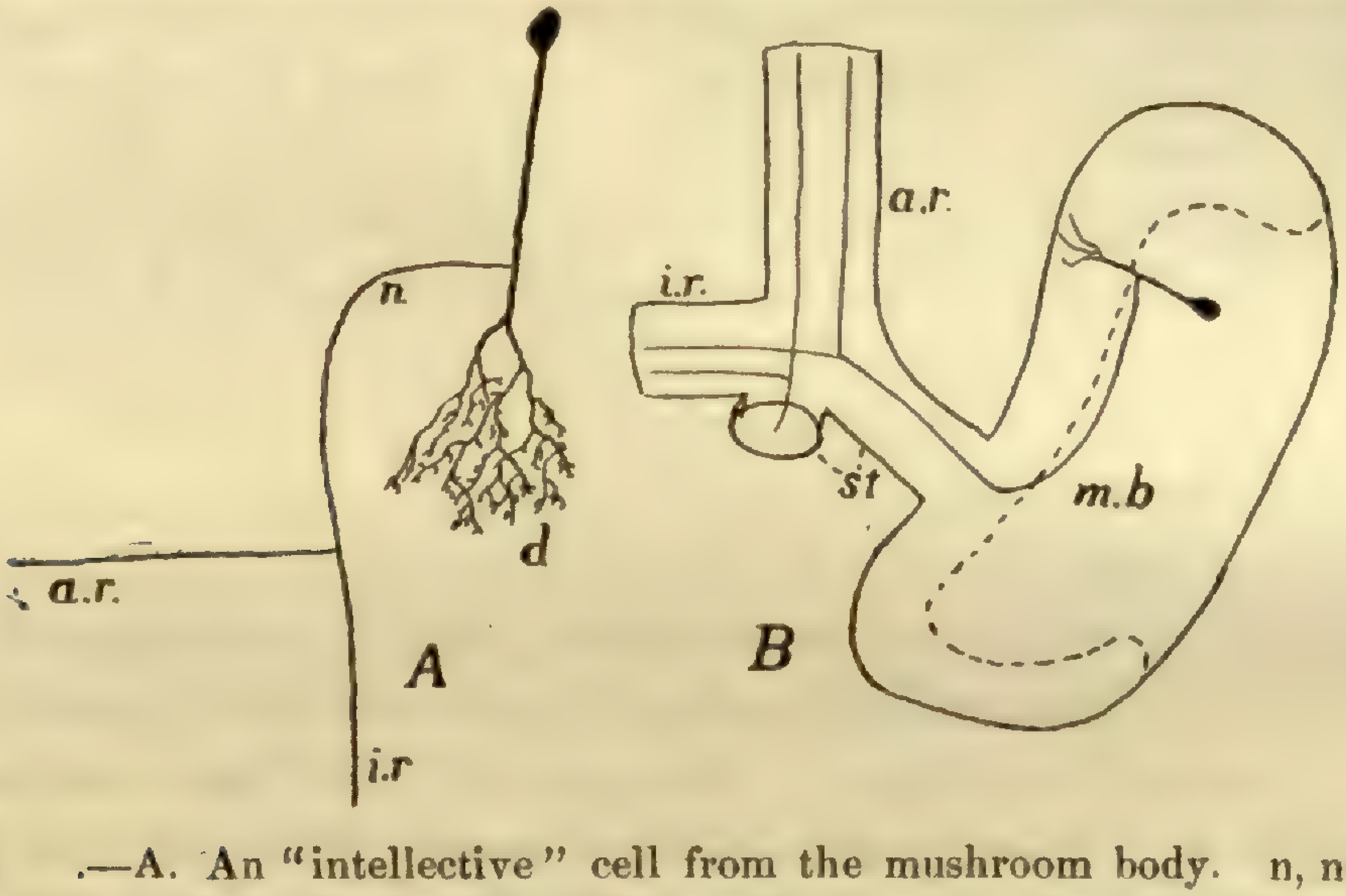

Fig. .—A. An "intellective" cell from the mushroom body. n, neurite; d. dendrite; a.r., anterior branch of the neurite; i.r., inner branch of the neurite.

B. Mushroom body of right side from above. The outer one, m.b, viewed in section; the inner one is cut off, leaving the stump of the stalk st. a.r., anterior root; i.r., inner root; m.b., cup.

The fibers branching among the parallel fibers of the roots and the stalk lead off to lower parts of the brain, connecting with efferent or motor fibers, or with secondary association fibers, that in their turn make such connections. This portion of the circuit has not been perfectly made out, though there seems to be sufficient data to warrant the assumption just made.

Such fibers existing as described there is then a complete circuit for sensory stimuli from the various parts of the body to the cells of the mushroom bodies. The dendritic or arborescent branches of these cells take them up and pass them on out along the parallel fibers or neurites in the roots of the mushroom bodies as motor or other efferent impulses.

This, however, is not all. For there are numerous fibers evident in my preparations, the full courses of which I have not been thus far able to determine, but which are so

situated as to warrant the inference that they may act as association fibers between the afferent fibers from the antennæ, optic ganglia, and ventral system and the afferent fibers. There is then a possibility of a stimulus entering the brain and passing out as a motor impulse without going into the circuit of the fibers of the mushroom bodies, or, in other words, a possibility of what may be compared to reflex action in higher animals.

It appears then that the supposition of Dujardin is well supported by the finer structure of the hexapod brain. For it is evident from the details known since the publication of Flögel's paper, that the cells composing the mushroom bodies have been very highly differentiated in some of the hexapods, and this in just those forms living the most complex lives. No such bodies are to be found in the lower forms. I have never seen them, nor any indication of them, in the Thysanura, Chilopoda,[2] Scolopendrella, the Pauropoda and other Myriapoda, nor in any of the Crustacea that I have thus far examined. Without doubt an application of the Golgi or methylen blue methods would reveal elements in some these forms that might be compared with the cells of the mushroom bodies; but they would probably be found not so completely differentiated from other fibers as they are in the honey bee and other Hymenoptera. It may be mentioned that one does not recognize such cells in the cray-fish and the crab as figured by Retzius and Bethe. And it scarcely need be said that no such elements are shown in Retzius' figure of the brain of *Nereis*.

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[2] St.-Remy ('90) describes mushroom bodies as occuring in *Scutigera*, which if homologous with the mushroom bodies of Hexapoda, is in accordance with Dujardin view.

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EDITOR'S TABLE.

The Zoological Section of the American Association for the Advancement of Science at its meeting in Springfield, Mass. in August, 1895, adopted a series of resolutions which are printed in the volume of the Proceedings recently issued (p. 159) and which are here reproduced. They were adopted with but one pertinent objection from a distinguished member of the section. This objection was that the method of determining priority of publication recommended in the resolutions was applicable to questions of nomenclature only, which was regarded as an object of a value secondary to the determination of date of discovery of matters of fact. While the fixing of date of the latter was admitted to be of great importance, it was contended by the friends of the resolutions, that the manner proposed by them was applicable to all possible cases, and that in fact the resolutions prescribed the best method of determination of priority. The mode proposed was stated to be in accord with that customary among authors and publishers generally, and that special groups of authors could not in practice sustain rules different from them. The resolutions are as follows.

Whereas: The date of publication is a question of fact to be determined by examination, and not by an arbitrary ruling: and

Whereas: In the world at large the date of publication of books is the date at which they are printed; and

Whereas: The adoption of any other date of publication would have no practical effect for this reason, and for the following additional reasons; viz.:

First; the majority of publications are not distributed, but are sold;

Second; the distribution when it occurs may be rendered ineffective by accidents such as loss of mails, fires, etc.;

Third; distribution by individuals may be delayed or prevented by absence from home, sickness or death;

Fourth; distribution by governments of their publications is often delayed for routine reasons;

Fifth; the actual date of mailing will be often impossible to ascertain with precision, owing to lack of record and irregularity in the period of transmission; and

Whereas: The determination of the date of printing will generally depend on the records of the printing office and the testimony of several persons, while the time of mailing will be known generally to but one person;

RESOLVED: *First.*—The section of Zoology of the American Association for the Advancement of Science recommends that the date of the completion of printing of a single issue be regarded as the date of publication;

Second.—That the Section recommends that such date be printed on the last signature of all publications, whether books periodicals or "separates."

RESOLVED: (1) That the Section of Zoology of the A. A. As. S. is impressed with the desirability of introducing the custom of placing all publications on record at some central agency together with the date of publication. (2) That a committee be appointed to obtain the approval of these resolutions by publishing societies at home and abroad. (3) That a copy of these resolutions be transmitted to the British Assoc. Adv. Science; the Zoölogical Society of London; Australasian Assoc. Adv. Science; Association Francaise; Société Zoologique de France; Versamml. der Deutscher Naturforscher, n. Aertzte; Zoologisches Gesselschaft; and the International Congress of Zoology held at Leyden.

To act as the committee above referred to, the President of the Section appointed: S. A. Forbes, Champaign, Ill.; E. A. Birge, Madison, Wis.; W. A. Lacy, Lake Forest, Ill.; George Dimmock, Canobie Lake, N. H.

The above resolutions were adopted by very large majority vote. A proposition to regard as the date of publication, the date of receipt at the central agency of record was introduced. This was not approved, as it was evident that no private arrangement made by naturalists could supersede the customs long since current in the world of authorship.

The American Association for the Advancement of Science has a peculiar custom which it seems to us might be improved. This is the use of the term vice-president to designate the presidents or chairmen of the respective sections. This expression gives use to confusion, as these officers are not the vice-presidents of the sections, but the presidents. If the expression vice-president of section so and so is used, a president is supposed, who does not exist. To avoid conflict with the title of the president of the Association, the term chairmen might perhaps be used for the so-called vice-presidents, but actual presidents of the sections.

The decimal system of record, called the Dewey system in library catalogues, appears to the management of the Naturalist to be the best method which has yet been devised. It, therefore, follows Natural Science and La Revue Scientifique in adopting it.

RECENT LITERATURE.

The Structure of Solpugids.—That indefatigable student of the Arachnida Mr. Henry M. Bernard has presented us with a valuable account[1] of the general structure of these little known forms. And yet while we can praise the statement of facts, as a whole, we would point out that the paper contains a number of theoretical points, which have, in our estimation, no sufficient basis.

The Galeodidæ, of which over 50 species have been described, are confined to the warm portions of both hemispheres, and though abundant in certain regions, they are comparatively rare in collections; possibly from the fact that they are, by popular consent, accorded most poisonous qualities. They, alone of all the Archnida, show a distinct "head" while they also have a "thorax" divided into three segments, and these points have led many authors to look upon them as forming a transition between the Archnida and the Hexapods. They also possess stigmata in the thoracic region, a condition only paralleled in the Arachnida in certain of the mites.

In his paper Bernard takes up first the external anatomy and the interesting features here are: the interpretation of the cephalic lobes as the lateral regions of the first segment which have been changed in position with the transfer of the cheliceræ; and he further tries to find them in the cephalic lobes of embryos of other Arachnids, a view with very little in morphology to support it. The beak is interpreted as fused labium labrum, neither of these, as the name of the first might imply, being appendicular in nature. The ocular tubercle is regarded as the only remnant of the original dorsal surface of the head, the rest having been displaced by the upward and backward movement of the cephalic lobes; and, from this, the median eyes are regarded as the more primitive, the lateral as secondarily acquired. The descriptions of the limbs, as well as of the apodematous skeleton affords little to abstract, except that the author suggests that since specialized poison organs are absent the poison may come from setal-pores on the cheliceræ; and that, at any rate, the idea of their poisonous nature should not be set aside without further experiment. As little need be said of the account of the hypodermis or of the muscular systems.

The account ef the nervous system is disappointing. Although sections were cut (cf., p. 345) no use of them appears to have been made

[1] Trans. Linn. Socy. London, Zool. Vol. vi, pt. 4, 1896.

in the study of the topography of the system and we are left absolutely in the dark as to the presence of ganglia in front of those of the cheliceræ; a point of no little importance. The eyes receive hardly more satisfactory treatment, owing to the unsatisfactory condition of the specimens. No vitreous body was found in the median eyes while the retinal cells showed no rods, and no grouping of these into a rhabdem was seen. The lateral eyes vary in size, shape, and arrangement and are described in some cases as having fused on either side of the head, although no evidence is presented of such fusion. The pedipalpal organs, reversible sacs on the tips of these appendages are described in detail and are clearly sensory as are the "racquet organs" on the last pair of thoracic appendages.

The alimentary canal opens by the mouth at the end of the beak, the opening being fringed with a strainer of bristles, while the œsophagus, in front of the œsophageal collar, is modified into a "sucking stomach." The midgut is provided with gland, like diverticula and although they are grouped into those of the cephalothorax and abdomen, all clearly belong to one series, but those of the abdomen are remarkable not only from the number but from the fact that they empty into a collecting duct on either side and these ducts, in turn, empty into the intestines near the base of the abdomen. The Malpighian tubules are well developed and are described as emptying into the midgut, and Bernard accepts the views of Loman that these organs in the Arachnids cannot be homologous with the similarly named structures in the Hexapods. The heart has retained 8 pairs of ostia, while there are indications of another segmental chamber in front. From in front an aorta carries the blood forward and "appears to discharge the blood directly on to the central nervous system. There are no indications of the circumneural vessels like those of the Scorpions and of which Mr. Bernard holds, in some respects, peculiar views.

The respiratory system affords more that is interesting. The observations of previous students that there are three pairs of stigmata (and sometimes a fourth unpaired) is confirmed. Of these the first pair open behind the coxæ of the second pair of legs while the others compare with the anterior pulmonary openings of the Scorpions. Arguing from the conditions of the blood-vessels (and more from his preconceptions of the phylogeny of the respiratory organs Bernard concludes that there were originally two other tracheal openings in the thorax. There then follow some interesting but inconclusive remarks upon the primary number of stigmata in different Arachnids. While dealing with these respiratory structures the author deals with the question of

the origin of tracheæ from lung books (p. 375) and accepts the view that the former were the more primitive, the latter secondary, and reinforces it with the remark that this view "arrived at by comparative morphology, has recently been confirmed by embryology. Janorowski has discovered that the tracheal invaginations of Spiders first from branched tracheal tubes and that the lung books are a secondary specialization." And this without the slightest reference to the results of Simmons (since amply confirmed by Purcell and Brauer) which are directly the reverse. It is to be said in passing that the thoracic stigmata of the Solpugidæ, like those of the Acarina, are the greatest difficulty presented to those who believe in the Limulus-Arachnid theory, but the author dismisses the results of Wagner in this connection with the remark "that all conclusions based upon transitional phenomena of single specialized types will have ultimately to be tested by a profounder and more extended comparative study of existing forms."

The coxal glands, naturally have much attention. The external opening occurs between legs 3 and 4, the duct is long and convoluted while the gland itself is described as a great mass of tubules. These organs he is still inclined to think the derivatives of setipareus sacs, a view which "has hitherto met with no favor." Regarding the fact that they may be cœlomic in character he merely refers to Lauries observations on the scorpion and says that until this be confirmed the bulk of evidence seems to point to the coxal glands as a blind ending tube. And again (p. 381). "I freely admit that these arguments would have but little weight as against direct embryological evidence, if that evidence were really satisfactory." Certainly the results of Grobben, Kishenonyi, Lebinsky, Kingsley and especially those of Brauer are confirmative of those of Laurie, all showing the coxal glands are derived from the cœlomic wall and are the purest of mesoderm (if there be such a layer) and that their external opening is a subsequent formation. For the opposite view, held by Bernard, there seems not the slightest evidence.

After a few remarks upon the genital organs the author presents an attempt to elucidate the phylogeny of the Arachnida, and it is here that we are most at variance with him. It is impossible to go into his argument in detail. It all rests upon the attempt to derive every existing Arthropod structure from structures already present in the annelid ancestor, setiparous sacs apparently playing the most important point. These coxal glands, tracheæ, poison glands, stink glands, spinning glands cement glands, maxillary glands, salivary glands, etc., are all referred

back to the setiparous gland of the annelid; yes further, the hairy bodies of the Solpugids and Mygalidæ are direct inheritances from the annelid setæ. Scorpio is not primitive but rather a specialized form. In some of his statements of fact he also seems to be in error. Thus he says (p. 398) "What actual evidence we have as to the character of the abdominal limbs [in the primitive Arachnid] shows that they were filamentous jointed appendages like those on the cephalothorax." On the contrary in Scorpions (cf. Brauer, Patten) which, with all deference to Mr. Bernard, we continue to regard the most primitive of existing Arachnids, they appear in the embryo as flat lamellate limbs. Again (same page) he says that the sensory plates on the pectines of the scorpion are on the ventral and not on the posterior face of the limb. On the contrary they are on the posterior side as the figures of both Patten and Brauer show. But what we have most to criticise is the failure to refer to opposing views or corrections of previous statements. Thus he refers to "stigmatic scars" along the whole length of the abdomen of the Pseudoscorpions, scars which bear another interpretation. He speaks of the entostemite as ectodermal, without stating that a portion of it is mesodermal (Schimkewitsch), while we have referred to other cases above.—J. S. K.

The Bears of North America.[1]—A new classification of the bears of North America is proposed by Dr. Merriam. This classification is based on the study of more than 200 skulls, including about 35 skulls of the huge bears of the Alaska coast region. The number of full species recognized by Dr. Merriam is ten: 4 of the Black Bear group; 2 of the Grizzly group; 3 of the big brown bears of Alaska, and the Polar bear. Four of these species are new; (1) the gigantic fish-eating bear of Kadiak Island and the Alaskan Peninsula, *Ursus middendorfii* Merr.; (2) the large brown bear of Yakutat Bay and the coastal slope of the St. Elias Alps, *Ursus dallii* Merr.; (3) the large brown bear of Sitka and the neighboring islands, *Ursus sitkensis* Merr.; and (4) the Florida Black bear, *Ursus floridanus* Merr. The Sonoran Grizzly and the Norton Sound Grizzly are considered as subspecies only. The Alaskan bears fall into 2 distinct groups. (1) *U. sitkensis* and *U. dallii*, which resemble the Grizzlies in the flatness of their skulls, but are larger and differ from them in color and dentition; and (2) *U. middendorffii* which differs markedly from all other American types, and closely resembles the Great Brown Bear of Kamschatka. Merriam's synopsis is illustrated by figures of the skulls of the different species.

[1] (Proceeds. Biol. Soc., Washington, April, 1896.)

As an account of the North American bears this paper is far in advance of anything hitherto published.

The difficulty of distinguishing several species of the typical Ursi in North America has not been so much the absence of characters among themselves, as the intermediate position of the old world *Ursus arctus* with regard to them. Middendorff's studies of this species convinced him that it varied in size 33 per cent. of the largest dimensions, and in other respects, but he could not refer the varieties to more than one species. With these very elaborate studies as a basis, J. A. Allen and A. E. Brown in subsequent years could only see in the North American grizzly and black bears, geographical races. The fault then of Dr. Merriam's paper is, that he has not given any account of the relations of our bears to the intermediate series of the Old World.

Dr. Merriam is a genus fancier, and he bids fair to adopt all of the names of his illustrious predecessor Dr. J. E. Gray of the British Museum. Thus he adopts Gray's name, Thalarctos for the polar bear on characters which do not exist. He dallies with Euarctos for our black bear for equally poor reasons. We must admit, however, that Dr. Merriam does for the first time give satisfactory characters with which to distinguish this species from the *Ursus arctus*.

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General Notes.

PETROGRAPHY.[1]

The Eruptives and Tuffs of Tetschen.—Two interesting articles on the area of crystalline rocks east of Tetschen on the Elbe, have appeared simultaneously. The first, by Hibsch, is a description of the Tetschen[2] sheet of the map of the Bohemian Mittlegebirges, and the second by Graber,[3] is on the fragments and bombs occurring in the tephrite tuffs of the region.

The volcanic rocks of the district are interbedded basalts, tuffites, tuffs and tephrites, of which the fragmental rocks are in greatest abundance. Augitites also occur as sheets, and camptonites as dykes in upper Cretaceous marls. The older igneous rocks are granitites and diabases that are associated with clay slates, probably of Cambrian age. Analyses of each of these rocks are given but the rocks are not described in detail. The greater portion of the author's article deals with the volcanic rocks. The tuffs are composed of basaltic and tephritic fragments of the coarseness of sand in some cases, and in others of

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Min. u. Petrog. Mitth., XV, 1895, p. 201.

[3] Ib., p. 291.

pieces several feet in diameter. These are cemented together by finer portions of the same substances, among which have been deposited zeolites, carbonates, opal and other secondary minerals. Some beds of this tuff are so filled with large fragments of basalt, tephrite, etc., that the rock composing it has been called the "Brocken Tuff." It is to the study of the fragments in this tuff that Graber's paper is devoted.

The basalts and tephrites constitute sheets and lava streams that are interstratified with the tuffs and sediments. Among the former rocks are noticed feldspathic, leucitic and nephelinic varieties, besides in several places magma-basalts. In addition to sheet basalts, dykes and chimneys of this rock have also been observed.

The rocks in all their forms are normal in their development. The author regards contact action around the chimneys as the safest criterion by which to distinguish these forms from denuded sheets and flows. The tephrites comprise hauyn-tephrites, in which hornblende and aegerine are present, nepheline-tephrite, including trachytic and andesitic varieties, and leucite-tephrite composed of phenocrysts of augite, plagioclase and grains of magnetite in a groundmass of these same components, and leucite, biotite and nepheline.

The augite consists of two generations of magnetite and augite in a glassy base. Its analysis gave:

SiO_2	TiO_2	P_2O_5	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	K_2O	Na_2O	H_2O	Moisture	Total
43.35	1.43	1.54	11.46	11.98	2.26	7.76	11.69	.99	3.88	2.41	.59	=99.34

The feldspathic basalt and the andesitic tephrite are the only rocks that seem to have affected the sediments with which they are in contact. Quartzites are changed to aggregates of quartz grains in a glass matrix, where the action is not extremely severe, and to an aggregate of interlocking quartz grains where it has been intense. The article closes with an account of the detailed results of analysis of ten specimens of the volcanic rocks.

Graber's article is devoted principally to a description of the fragments found in the Brocken-tuff. These are all tephritic rocks, among which andesitic, leucitic and phonolitic types are recognized. The characteristics of the components of all these types are portrayed in great detail, especial care being given to the descriptions of the augite and the plagioclase. The phonolitic tephrite is characterized by the presence of nosean, which is in irregular grains. In the andesitic tephrite, which is the most basic variety, the porphyritic augite has an extinction angle $c \wedge C$ of 58°–62°, in the leucitic type its extinction is

52°–56° and in the phonolitic type, the most acid variety, it is 50°–53°. In each of the types labradorite and sometimes oligoclase phenocrysts are common, but the feldspar of the groundmass differs in character in the different types. In the andesitic type it is oligoclase, in the leucite variety andesine, and in the phonolitic type sanidine.

A Nepheline-Syenite Bowlder from Ohio.—Miss Bascom[4] has found in the drift near Columbus, Ohio, a bowlder which consists of nepheline-syenite porphyry. The rock is composed of large phenocrysts of oligoclase and smaller ones of nepheline, augite, hornblende and olivine in a groundmass composed of plagioclase and orthoclase laths, hornblende, biotite, augite and magnetite in a feldspathic matrix.

Crystalline Rocks of New Jersey.—In a report on the Archean Highlands of New Jersey, Westgate[5] states that the northern half of Jenny Jump Mt., Warren Co., consists mainly of gneisses with a small area of crystalline limestone, diorites, gneisses, etc. The gneisses are granitoid biotite-hornblende varieties, biotite-gneisses and hornblende-pyroxene gneisses. In the first named variety the prevailing feldspars are microcline and microperthite, and in the pyroxene gneisses plagioclase and orthoclase. The gneisses are cut by pegmatite dykes, amphibolites and diabases.

Associated with the white crystalline limestones are fibrolite and biotite gneisses, hornblendic gneiss, amphibolites, gabbros, norites and diorites, most of the latter of which show evidence of an eruptive origin. Another type of rock often found associated with the limestones is a quartz-pyroxene aggregate, in which the pyroxene is a green or white monoclinic augite. The limestone, the fibrolite and biotite gneisses and the quartz-pyroxene rock are thought to be metamorphosed sediments.

Simple Crystalline Rocks from India and Australia.—Judd[6] gives us an account of several simple crystalline rocks from India and Australia. One is a corundum rock composed principally of corundum grains with rutile, picotite, diaspore and fuchsite as accessory constituents. The corundum is in part pale colored and in part strongly pleochroic. The grains of the latter extinguish together producing with the former a micro-poicilitic structure. One of the specimens examined came from South Rewah and the other from the Mysore State.

[4] Journ. Geol., Vol. IV, p. 160.

[5] Ann. Report State Geol. of New Jersey for 1895. Trenton, New Jersey, 1896, p. 21–61.

[6] Mineralogical Magazine, Vol. XI, p. 56.

Associated with the corundum in the Mysore State is a fibrolite rock. A tourmaline rock from the Kolar gold field in the same State and from North Arcot and Salem in Madras, consists of twisted and bent tourmaline fibres in a matrix of smaller fibres of the same substance. In the neighborhood of Bingera, New South Wales, two rocks are found as dykes cutting serpentine. One consists almost exclusively of green garnets and the other of picotite. The former contains also gold and chrysocolla.

The Weathering of Diabase.—Mr. Merrill[7] describes the changes that have been effected in a granular diabase at Medford, Mass., during its disintegration into soil. Bulk analysis of the fresh and the weathered rock yielded the following results:

	SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	MnO	K_2O	Na_2O	P_2O_5	Ign	Total
Fresh	47.28	20.22	3.66	8.89	7.09	3.17	.77	2.16	3.94	.68	2.73=	100.59
Weathered	44.44	23.19		12.70	6.03	2.82	.52	1.75	3.93	.70	3.73=	99.81

The disintegration of the rock is accompanied by a leaching out of its most soluble constituents. Assuming that the alumnia has remained unchanged in quantity in the course of the disintegration, the percentage of each constituent lost in this process is shown to be as follows:

SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	MnO	K_2O	Na_2O	P_2O_5	Ign
18.03	.00	18.10		25.89	21.70	41.57	29.15	12.83	11.39	.00

The paper is full of valuable suggestions that cannot be even referred to in these notes.

Petrographical Notes.—Transitions from massive anorthosites into augen gneisses and into thinly foliated gneisses and transitions from olivine gabbro into hornblende schists are briefly described by Kemp[8] in a preliminary article on the dynamic metamorphism of anorthosites and related rocks in the Adirondacks.

Pirsson[9] suggests the use of the word anhedron to express the meaning usually expressed in the phrase 'hypidiomorphic form.' An anhedron is a body with the physical constitution and properties of a crystal but without the crystallographic form. The term may be conveniently applied to the crystalline grains in rock masses.

[7] Bull. Geol. Soc. Amer., Vol. 7, p. 349.
[8] Bull. Geol. Soc. Amer., Vol. 7, p. 488.
[9] Ib., Vol. 7, p. 492.

GEOLOGY AND PALEONTOLOGY.

The Limestones of the Jenny Jump Mountains, New Jersey.—Accompanying the report on the Archean Geology of New Jersey, by Mr. J. E. Wolff is a paper by Mr. L. G. Westgate on the Geology of Jenny Jump Mountain, chiefly interesting on account of the conclusions reached by the author concerning the crystalline limestones of that region.

The area under consideration embraces the northern half of Jenny Jump mountain in Warren county, New Jersey. This mountains lies along the northwestern border of the highland area, and is a sort of outlier or peninsula reaching into the later Paleozoic rocks. The main ridge of the mountain consists of gneisses; the limestone occurs at its extreme northeastern end, with outcrops along the southeast border of the mountain.

The author discusses in detail the position, lithology and relations to the crystalline limestones in other parts of New Jersey, and reviews the views of previous writers as to the age of the Sussex county limestone, which has generally been considered the type and representative of other localities. Mr. Westgate's views are given in the following summary:

"The crystalline limestones of Warren county are believed to be distinct from and older than the blue magnesian limestone of Cambrian age, which occurs along the northwestern side of the New Jersey Highlands. They are believed to be distinct, for the following reasons."

"1. They differ lithologically from the blue limestone in being thoroughly crystalline, and in containing large amounts of accessory metamorphic minerals."

"2. They are intimately associated with and apparently interbedded with the older gneisses; and gneisses occur also interbedded in the limestone."

"3. They show no intimate association in areal distribution with the blue limestone, nor any tendency to grade into it."

"4. The metamorphic changes to which the white limestones have been subjected are general in their nature, and are not due to the action of the eruptives by which they are cut; so that no sufficient agent is at hand to account for the supposed change from blue into white limestone."

"The white limestones are believed to be older than the blue Cambrian limestone, because (1) they occur in intimate association with the gneisses which are of admitted pre-Cambrian age, and because (2) they have been subjected to general metamorphic forces resulting in great changes, of which the neighboring blue limestone shows no traces."

"That the other crystalline limestones of New Jersey are of the same age as those of Warren county, has not been proved. The theory has generally been that they are. If they are, and if the position taken in the present paper is valid, then the crystalline limestones of Sussex county, and of other places in New Jersey, would also be, as they have generally been supposed to be, of pre-Cambrian or Archean age." (Ann. Rept., New Jersey State Geologist for 1895. Trenton, 1896.)

Unios from the Trias.—Four new Triassic Unios are described by Mr. C. T. Simpson. The collection of which they form a part was obtained from the Dockum beds, a formation underlying the Staked Plains of Texas. Taken as a whole, these Unios closely resemble in form, and are apparently nearly related to those of the Jurassic beds of North America, while 3 of the species bring to mind most strongly the species which now inhabit Europe and western Asia, and a small group belonging to the Mississippi area. The variety of characters displayed by these Triassic Unios go to show that the genus must have been well established at the time the Dockum beds were laid down, thus tending to overthrow Neumayer's theory that the Unionidæ were derived from the genus Trigonia, which probably does not date back to a period earlier than that of the shells under consideration. (Proceeds. U. S. Natl. Mus., Vol. XVIII, 1895.)

The Cadurcotherium.—M. Boule calls attention to the recent discovery of the lower jaw of a Cadurcotherium (Gerv.) at Barlière (Haut-Loire). The specimen denotes an animal of the size of a small rhinoceros. It was found in oligocene arkoses associated with a fine mandible of *Elotherium magnum*, and fragments of Aceratherium, and the remains of turtles. Until now Cadurcotherium has been represented by isolated teeth and fragments of mandibles. The new find is important, showing the animal to be unique among its contemporaries. It presents certain resemblances to South American types—noticably Astrapotherium of the Patagonian Eocene, but is, according to Osborn really related to the rhinocerontic genus Metamynodon.

Notes on the Fossil Mammalia of Europe, V—The Phylogeny of Anoplotherium.—The early attempts at the construction of a phylogeny of the even-toed ungulates, included the genus *Anoplo-*

therium, which was considered by Paleontologists of twenty-five years ago, as a primitive form, especially in its foot structure, *Anoplotherium* certainly possesses a number of primitive characters in its manus and pes, such as the separation of the metatarsals, and the non-fusion of the podial elements, but the inadaptive reduction of its digits, as pointed out by Kowalevsky and the peculiar position of the pollux and hallux, excludes the possibility of placing *Anoplotherium* in the direct line leading to any of the living Artiodactyla.

I propose in this short paper to attempt to prove, that *Anoplotherium* has been probably derived from *Dacrytherium*, a closely allied genus, but whose foot structure is normal and which resembles that of many of the early Eocene Artiodactyla such as *Cainotherium*. Prof Cope[1] suggested that *Cebochærus* may have been the ancestor of *Anoplotherium*, but the structure of the skull in *Cebochærus*, is already quite modernized, nearly as much so as in the true pigs, consequently I am inclined to think that we shall have to look for some other form as ancestral to *Anoplotherium*.

The general form of the skull in *Dacrytherium* is like that of *Anoplotherium*, however, in *Dacrytherium* there is a strongly pronounced preorbital fossa, which is absent in *Anoplotherium*. The crowns of the upper teeth in *Dacrytherium* are low and primitive in structure. They exhibit rounded external crescents, which are not at all angular. In *Anoplotherium*, especially the large species, the crowns of the superior true molars are more lengthened than in *Dacrytherium* and the external crescents are angular and broad. We see this change in many mammalian phyla from extremely low crowned molars, to those which are tending to the hypselodont condition. As regards the intermediate stage, between *Dacrytherium* and *Anoplotherium*, as to the height of the molars, this is found in the genus *Diplobune*.

The lower true molars of *Dacrytherium* exhibit two internal cones, which is the normal number in the Artiodactyla. It is interesting to record, that I have noticed in a number of young jaws of *Dacrytherium* in which the true molars were just coming through, that the antero-internal cusp, which is single in the adult, shows a slight reduplication, which is the normal condition in *Diplobune*. The division of the metaconid is carried still further in the largest species of *Anoplotherium*, although I have examined many jaws from the Phosphorites of the *Anoplotherium*, and I can confidently state, that all gradations exist between the complete isolation of the two antero-internal cusps of the typical forms of *Anoplotherium*, and the single condition of these cusps,

[1] Artiodactyla, AMERICAN NATURALIST, Dec., 1888, p. 1083.

which is found in the supposed ancestral genus, *Dacrytherium*. Accordingly I am not acquainted with any good generic character at present, which will distinguish the so-called genus *Diplobune* from *Anoplotherium*, as in many cases in jaws from the Phosphorites, it is impossible to say whether they belong to *Anoplotherium* or *Diplobune*. Dr. Henri Filhol informed me that he was of the same opinion, in regard to the validity of the genus *Diplobune*.

In Dacrytherium the hind foot has at least four well developed toes and the internal digit is not placed at an angle with the others as in *Anoplotherium*. This structure of the pes is just what one would expect to find in a genus standing in ancestral relationship to the more specialized members of the *Anoplotheriidæ*. Granting that *Dacrytherium* fulfills in most of its characters, what we require of a form, supposed to be ancestral to *Anoplotherium*, there is still the presence in *Dacrytherium* of a preorbital fossa, which is absent in the skull of *Anoplotherium*, and also another objection, is, that *Dacrytherium* has claw-like ungual phalanges, much as in *Agriochœrus*. I believe, however, the extremely compressed ungual phalanges of *Dacrytherium* is of little weight against this genus being ancestral to *Anoplotherium*, for in the latter these phalanges are rather compressed, more so than in the normal Artiodactyles, and they could be easily derived from those of *Dacrytherium*. The structure of the skull is not known in all the species of *Anoplotherium*, and one of them may have had a skull with a preorbital fossa, which is so characteristic of *Dacrytherium*.

As is well known, the original specimens of the manus and pes of *Anoplotherium commune*, which are in the Muséum d' Histoire Naturelle, Paris, show only two well developed digits as restored by Cuvier. This restoration of the feet of *Anoplotherium* is shown by Schlosser and Zittell to have been an error on the part of Cuvier, and I quite agree with these authors on this point. Prof. Zittell in his "*Traité de Palæontologie*" in speaking of the structure of the feet in *Anoplotherium* remarks "La plupart des représentation de la patte d' *Anoplotherium* faites jusqus à present omettent par erreur à la patte antérieur l' index et le rudiment de pouce, à la patte postérieur le second doigt." I have examined a fine cast of the hind foot of *Anoplotherium commune* and I find that the restoration of the internal portion as completed by Cuvier is quite erroneous. The two small bones placed by him on the tibular side of the pes do not at all fit the facets on which they are placed. The broad and obliquely placed facet on Mt. 111 in *A. commune* is for the large and wide spreading second digit, this same structure of the metatarsal occurs in *A.* (*Eurytherium*) *latipes* of the upper Eocene of Débruge.

Summing up the principal changes which have occured in the evolution of *Anoplotherium* from *Dacrytherium*, I emphasize the following: 1. Increase in height of the crowns of the upper molars, and the reduplication of the metaconid of the lower molars, this division of the metaconid is found in an incipient condition in young jaws of *Dacrytherium*. Complete separation of the metaconid into two distinct cusps only occurs in some forms of *Anoplotherium*. 2. The hind foot of *Dacrytherium* is normal in structure, and has at least four toes, this is the primitive type of pes, from which the specialized foot of *Anoplotherium* has been derived.

Note.—In my "Notes on the Fossil Mammalia of Europe," part III, AMERICAN NATURALIST, April, 1896, I find two mistakes, which should be corrected. On page 309, third and fifth lines from top, read *Adriotherium*, instead of *Adiotherium* as printed, and also page 310, eighth line from the bottom, read *Anoplotheriide*, in place of Suillines. —CHARLES EARLE.

BOTANY.[1]

De Toni's Sylloge Algarum.—Dr. De Toni[2] has recently issued the third volume of his *Sylloge Algarum*. It deals entirely with the Brown Algæ or *Phæophyceæ*—the FUCOIDEÆ as he calls them. A thousand species are described under one hundred and eighty genera, which are grouped into twenty-nine families. He divides the group into three orders, *Cyclosporinæ*, (Fucaceæ) *Tetrasporinæ* (Dictyoteæ) *Phæozoosporinæ* (Phæozoosporeæ).

Splanchnidium rugosum the interesting plant which after careful study was placed by M. O. Mitchell and F. G. Whiting[3] in the *Phæosporinæ*, is retained in the *Durvilleaceæ*, the fruit being described as a polysporous oogone. The general appearance of the plant and the structure of the conceptacles suggest a close relationship with the fucoids, but if the above investigations are to be accepted the plant

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

[2] *Sylloge Algarum Omnium Hucusque cognitarum* by J. Bapt. De Toni, Vol. III, *Fucoideæ*.

[3] On Splanchnidium rugosum Grev. the type of a new order of Algæ, Phycological Memoirs, Pt. I. I., 1892.

bears zoospores in the conceptacles and not oogones, hence it must be placed in the *Zöosporinæ.*

The treatment of the Zoosporinæ is practically that of Kjellman in Engler and Prantl's, *Pflanzenfamilien*, except that the genera *Lithoderma* and *Arthrocladia* are placed in families by themselves, instead of in the *Ralfsiaceæ* and *Desmarestiaceæ* respectively, and that De Toni has included five small, mostly, monogeneric families, the *Phæothamniaceæ, Phæocapsaceæ, Hydruraceæ, Chromonodaceæ* and *Chromophytonaceæ* not mentioned by Kjellman. In all the Zöosporinæ except the above families the zöospores as far as known are laterally biciliated and are borne in some form of zöosporangia. In these families there are no zoosporangia and in at least a part of them the zoospores are not laterally biciliated and in general their relationship seems to be with the *Chlorophyceæ.* It seems more natural to place them, as Wille has with some of them, in the *Chlorophyceæ* next to their closely related genera.

The book is well arranged; priority in class, ordinal and family. nomenclature is strictly observed. It will be indispensible to the specialist in this line and a great help to the general student.—De Alton Saunders.

The Flora of the Black Hills of South Dakota.—In a recent number of the Contributions from the U. S. National Herbarium (Vol. III, No. 8; issued June 13, 1896), P. A. Rydberg gives the results of his explorations (in 1892) of the Black Hills of South Dakota. The report, which includes about eighty pages, includes the following, viz.: Itinerary, Geography, Geology, Altitudes, Precipitation and Temperature, Floral Districts, General Remarks, and the Catalogue of Species. The plates are a Map of the Black Hills, *Aquilegia brevistyla, Aquilegia saximontana* and *Poa pseudopratensis.* The floral districts recognized by the author are five, viz.: (1), the foothills and surrounding plains, (2), the Minnekata Plains, (3), the Harney Mountain Range, (4), the Limestone District, (5), the Northern Hills.

In summing up his discussion of the vegetation of these districts the author says, "From the foregoing can be seen what a varied flora the Black Hills have. There are found plants from the East, from the Saskatchewan region, from the prairies and table-lands west of the Missouri River, from the Rocky Mountains, and even from the region west thereof. In the foothills and the lower parts of the Hills proper the flora is essentially the same as that of the surrounding plains, with an addition of eastern plants that have ascended the streams. In the higher parts the flora is more of a Northern origin. Most of the plants

composing it are of a more or less transcontinental distribution, but often characteristic of a higher latitude. Some can be said to belong to the Rocky Mountain Region. The only trees of western origin are *Pinus ponderosa scopulorum*, and *Betula occidentalis;* the others are eastern, or transcontinental. The flora resembles, therefore, more that of the region around the Great Lakes than that of the Rockies."

It merely remains to say that the nomenclature and capitalization (*all* specific names decapitalized) of this interesting and valuable report are of the most advanced type.—CHARLES E. BESSEY.

Trelease's Hickories and Walnuts of the United States.—Dr. Trelease has rendered a good service to the botanists of the country by publishing (in the Seventh Annual Report of the Missouri Botanical Garden) the results of his studies of the Juglandaceæ of the United States, especially with reference to their winter characters. The species recognized are:

Hicoria pecan (Marshall) Britton.—Iowa to Southern Indiana, Kentucky, Louisiana and Texas, extending into Mexico.

H. myristicæformis (Michx. f.) Britton.—Arkansas to Alabama, Texas and Mexico, and in South Carolina.

H. aquatica (Michx. f.) Britton.—Virginia to Florida, around the Gulf to Texas, thence north to Arkansas and southern Illinois.

H. minima (Marshall) Britton.—Canada and Maine to Minnesota and Nebraska, south to Texas and Florida.

H. glabra (Miller) Britton.—Atlantic region from Massachusetts and Pennsylvania to Florida.—var. *odorata* (Marshall) Sargent.—Mississippi valley eastward, and from Canada to the Gulf.—var. *villosa* Sargent.—Missouri, on flinty hills.—var. *microcarpa* (Nuttall) Sargent.—Same range as var. *odorata.*

H. alba (L.) Britton.—Canada to the Great Lakes and Kansas, south to Texas and Florida.

H. mexicana (Engelm.) Britton.—Mexico, in mountains of Alvarez.

H. luciniosa (Michx.) Sargent.—New York and Pennsylvania to Iowa, Kansas and the Indian Territory.

H. ovata (Miller) Britton.—Canada to Minnesota, south to Florida, Kansas and Texas.

Juglans cinerea L.—New Brunswick to Dakota, Kansas, and the Mountains of Georgia and Alabama.

J. rupestris Engelmann.—Texas, New Mexico and Arizona, extending into Mexico.

J. californica Watson.—Coast range of southern California.

J. nigra L.—Massachusetts to Ontario and Minnesota, south to the Gulf.

The paper is accompanied by twenty five plates of trees, bark, buds, leaves and fruits.—CHARLES E. BESSEY.

Diseases of Citrous Fruits.—This recently issued bulletin (8) of the Division of Vegetable Pathology, of the U. S. Department of Agriculture, prepared by W. T. Swingle and H. J. Webber is a valuable contribution to science as well as horticulture. The diseases discussed are Blight, Die-back, Scab, Sooty-mold, Foot-rot, and Melanose. Eight good plates (three colored) accompany the paper.

Mulford's Agaves of the United States.—In the seventh volume of the annual report of the Missouri Botanical Garden, Miss A. Isabel Mulford publishes a monograph of the genus *Agave* so far as the species native to or growing spontaneously in the United States, are concerned. Sixteen species and four varieties are recognized, distributed as follows:

A. virginica L.—Maryland to Florida, Indiana, Missouri and Texas.
A. virginica var. *tigrina* Englem.—South Carolina.
A. variegata Jacobi.—Lower Rio Grande Valley, Texas.
A. maculata Regel.?—southern Texas.
A. schottii Engelm.—southern Arizona.
A. schottii var. *serrulata* n. var.—Rincon Mts., Arizona.
A. parviflora Torrey.—Mts. of Arizona.
A. lechuguilla Torrey.—west Texas and east New Mexico.
A. utahensis Engelm.—Utah, northern Arizona, southern California and Nevada.
A. deserti Engelm.—southern California.
A. applanata Lemaire.—western Texas.
A. applanata var. *parryi* (Engelm.)—southern New Mexico to central Arizona.
A. applanata var. *huachucensis* (Baker).—Huahuca Mts., Arizona.
A. shawii Engelm.—southwestern California.
A. palmeri Engelm.—southeastern Arizona and southwestern New Mexico.
A. asperrima Jacobi.—Spontaneous near San Antonio, Texas.
A. americana L.—Spontaneous in southern Texas.
A. rigida sisalana Engelm.—Naturalized in Florida.
A. decipiens Baker.—southeastern Florida.
A. sp.—Florida.
A. sp.—Texas.

It is with great pleasure that we observe the great reluctance of the author to establish new species; on the contrary she has refrained from giving names where most monographers would certainly have done so. Thus on page 96, after a description which might have been considered adequate, (at least by those who are fond of seeing their names cited in connection with specific names) the author says: "To avoid further confusion in nomenclature I refrain from giving a name to this plant until it is possible to obtain further data." We would commend this sentence to the careful consideration of a certain class of botanists who are apparently more anxious for their own "credit" than for the progress of the science.

Thirty eight plates, many of them half-tone reproductions of photographs, accompany this useful paper. If space permitted we should be glad to quote from the author's introductory discussion, which is full of interesting facts and suggestions; thus a case is cited in which the flower-stalk grew for twenty days at the average rate of two and three-fourths inches per day!—CHARLES E. BESSEY.

ZOOLOGY.

Sense of Sight in Spiders.—A detailed account of the experiments conducted by G. W. and E. G. Peckham for testing (1) the range of vision and (2) the color sense of spiders is published in a late volume of the Trans. Wisconsin Academy. The evidence offered by the authors is based upon a study of twenty species of Attidæ. This study has extended over eight successive summers, during which notes were made of many hundreds of observations. The movements and attitudes of the spiders of the group chosen are wonderfully vivid and expressive. The males, in the mating season, throw themselves into one position when they catch sight of a female, and into quite another at the appearance of another male. This power of expression through different attitudes and movements is of great assistance in determining not only its range of sight, but also its power of distinct vision.

The spiders were confined in boxes, the sides of which were marked off into inches. The bottom was of cotton cloth, the top of glass. Notes were taken of the distances at which prey was noticed, followed and captured. During their mating season the evidence was conclusive that these spiders not only see, but see clearly at considerable distance. The

following description of one of the many experiments described in the article serves to show the method of investigation:

A male of *Saitis pulex* was put into a box containing a female of the same species. "The female was standing perfectly motionless, twelve inches away, and three and a half inches higher than the male. He perceived her at once, lifting his head with an elert and excited expression, and went bounding toward her. This he would not have done if he had not recognized her as a spider of his own species. When four and one-half inches from her he began the regular display of this species, which consists of a pecular dance. This he would not have done had he not recognized her sex."

At another time a male of *Hasarius hoyi* was dropped into a box with another male which was standing seven inches away. "He at once threw up his first legs, this being a challenge to battle. The other male responded by throwing up his first legs. The two advanced upon each other slowly, and when only two inches apart began to circle about each other, waving their legs. The same male when put into a box with a female saw her as she stood quite eleven inches away, and at once lifted his first legs, not straight up, as in the case with the other male, but obliquely, and began to move with a gliding gait from side to side, this being the characteristic display before the females in this species."

That the spiders recognize each other by sight and not by any other sense is evidently shown by the fact that they remain unconscious of each other's presence when back to back, no matter how excitable they are when they come within range of each other's vision. As a further evidence of recognition by sight a male of *Dendryphantes elegans* was removed from the box in the midst of his courtship of a female, his eyes gently blinded with paraffine, and then restored to the box. He remained entirely indifferent to the presence of the charmer that had so much excited him a few moments before.

To sum up the result of these experiments:

"The Attidæ see their prey (which consists of small insects) when it is motionless, at the distance of five inches; they see insects in motion at much greater distances; they see each other distinctly up to at least twelve inches. The observations on blinded spiders, and the numerous instances in which spiders were close together, and yet out of sight of each other, showing that they were unconscious of each other's presence, render any other explanation of their action unsatisfactory. Sight guides them, not smell."

As to a color-sense in spiders, the authors are of the opinion that their experiments, while not conclusive, yet all taken together, strongly indicate that spiders have the power of distinguishing colors. (Trans. Wisconsin Acad. Sciences, Vol. X, 1895.)

Classification and Geographical Distribution of the Naiades.—In his study of the fresh water pearly muscles, Mr. Simpson finds that the division of these mollusks into two families, Unionidæ and Mutelidæ, founded on the completeness or incompleteness of the development of the siphons, cannot stand. He accordingly diagnoses the two families on the basis of the shell characters, and finds that his distinctions fully agree with what is known of the facts of geographical distribution of the paleontology of the Naiades, and the classification of v. Ihering, based on the characters of the embryos. The Unionidæ, as defined by the author, include the genera Unio Retzius, Anodonta Lamark, Prisodon Schumacher, Tetraplodon Spix, Castalina v. Ihering, Burtonia Bourguignat, Arconaia Conrad, Cristaria Schumacher, Lepidodesma Simpson, Pseudodon Gould, Leguminaia Conrad and Solenaia Conrad. In the Mutelidæ he places the following genera:—Mutela Scopoli, Chelidonopsis Ansey, Spatha Lea, Pliodon Conrad, Brazzæa Bourguignat, Glabaris Gray, Iheringella Pilsbry, Monocondylæa d'Orbigny, Fossula Lea, Mycetopoda d'Orbigny.

The author considers the relationship between these two great groups as not a very close one. The Unionidæ are characterized by schizodont teeth and a *glochidium* embryo. The Mutelidæ have taxodont teeth, and, so far as is known, the embryo is a *lasidium*.

Mr. Simpson finds that the Naiades are capable of being grouped into assemblages of related forms which have a more or less immediate common ancestry; and on the basis of this grouping they are distributed into eight provinces, as is shown in the following table:

Palearctic,	Europe. Northern and Western Asia. North Pacific to the Desert. Pacific drainage of North America.
Ethiopian,	Africa south of the Sahara.
Oriental,	Asia south of the Himalayas. East Indies to the Solomon Islands.
Australian,	Australia. Tasmania. New Zealand.
Neotropical,	South America.

Central American, . .	Central America. Mexico east of the Cordillera. Cuba.
Mississippian, ...	Entire Missisippi Valley and the Gulf drainage from West Florida to the Rio Grande. Mackenzie River system. Red River of the North. Great Lakes.
Atlantic, ...	Lower St. Lawrence and rivers of eastern Canada. Atlantic drainage of the United States.

The Unios date back in America to the Trias, where they were first discoved by Prof. E. D. Cope. The relations of the existing Naiad fauna with the fossil forms is given by the author as follows:

"The post Cretaceous Unios of the northwestern States is evidently closely related to the fauna of the Mississippi Valley, and this seems to be related to that of Tropical Africa, as well as to the tertiary forms of eastern Europe and Siberia. The Unios of Australia and South America are apparently closely related to those of the Australian region. There seems to be, too, a general relationship between the Mutelidæ of Africa and South America. These Mutelids and the Unios which bear the embryos in the inner gills have perhaps formerly occupied extensive areas in the northern hemisphere, and may have been supplanted by more modern forms." (Proceeds. U. S. Natl. Mus., Vol. XVIII, 1896.)

Arkansas Fishes.—As the result of less than three weeks' collecting in western Arkansas, eastern Indian Territory and the St. Francis River in northeastern Arkansas, Prof. Meek obtained 83 species of fishes. A new Notropis was found in the Potean River, and a new species of Fundulus is described from the St. Francis. Mollusks are abundant in old river, the old channel of the St. Francis. Six species of Unionidæ were found at a locality farther north than hitherto reported. (Bull. U. S. Fish Commission for 1895, Wash., 1896.)

Batrachia and Reptilia ot Madagascar.—The two collections of reptiles from Madagascar, now in the Natural History Museum of Paris, have been examined by M. Mocquard, who reports upon them as follows: The Grandidier collection comprises 68 species in all, Ophidians 13, Bathrachians 20, of which 3 are new species belonging to the genera Mantidactylus, Rhacophorus and Calophrynus. Lacertilians 35, including 2 new species, referred to the genera Lygodactylus

and Phyllodactylus. The Allnand and Belly collection comprise 33 Reptiles and 16 Batrachians. Among the latter are 2 new species of Mantidactylus and 1 of Stumpffia. There are but 11 Sphidia, but these include types of two new genera, Compsophis and Alluondina and a new species of Pseudoxyrhopus. The Lacertilia, 22 in number, yield 4 new species referred to the following genera: Chameleon, Brookesia, Uroplates and Paracontias. The diagnosis of the new Reptiles of this collection have been previously given in the *Comptes rendus de la Soc. Philom.* for 1894.

A comparison of these two collections, with the forms described by Prof. Boettger from Madagascar, shows that certain species considered by him as peculiar to Nossi-Bé are found distributed all through the northern part of the island. This is true not only of the Reptiles but of the Batrachians also. (Bull. Soc. Philom., Paris, 1895.)

The Molting of Birds.—In a paper published recently in the Proceeds. Phila. Acad., Mr. Witmer Stone gives a detailed account of his observations on the molting of birds, with especial reference to the plumages of the smaller land birds of eastern North America. Attention is directed to the following points: order, number and times of molt; change of color by abrasion; seasonal plumages; direct change of color in feathers. As a result of his studies Mr. Stone makes the following generalizations:

I. The annual moult at the close of the breeding season is a physiological necessity, and is common to all birds.

II. The spring molt and striking changes of plumage effected by abrasion are not physiological necessities, and their extent is dependent upon the height of development of coloration in the adult plumage, and does not necessarily have any relation to the systematic relationships of the species.

It naturally follows that closely related species may differ materially in the number and extent of their molts, and that males and females of the same species differ greatly in this respect when the nuptual plumage of the adult male is highly developed as compared with that of the female or with its own winter plumage.

III. The amount of change effected in the plumage at any particular molt varies considerably in different individuals of the same species and sex.

IV. Some species which have a well marked spring molt in their first and second years may discontinue it afterwards, when the adult plumage has once been acquired. And, on the other hand, some indi-

viduals may continue to molt in the spring, while others of the same species cease to do so.

V. The remiges are molted less frequently than any other part of the plumage. As a rule, they are only renewed at the annual molt (exception Dolichonyx).

VI. Variability in the order of molt in the remiges and presence or absence of molt in the flight feathers at the end of the first summer are generally family characters, i. e., Ceryle differs from any other species treated of in this paper in the order of molt in the primaries. All Picidæ and all Icteridæ, except Icterus (and Dolichonyx ?), molt the flight feathers with the rest of the first plumage. None of the Oscines except Icteridæ (as above), some (all ?) Hirundinidæ, Olocoris and Cardinalis molt the flight feathers at this time.

Mr. Stone's conclusions as to "color-change without moulting" are the same as those reached by Chapman, in his article on "The Changes of Plumage in the Dunlin and Sanderling," namely: that color-change without molt or abrasion is incapable of taking place from the very nature of the structure of a feather, and that all the cases so reported can be otherwise accounted for. (Proceeds. Acad. Nat. Sciences, Phila., 1896.)

The Florida Deer.—The fact that the Florida deer is but little more than half the size of the deer of northeastern United States, together with certain cranial and dental peculiarities, is sufficient, according to Mr. Outram Bangs, to give it full specific rank. He therefore describes it under the name *Cariacus osceola*. The most striking differences between the Florida animal and its northern relatives are (1) the shape and size of the nasal and maxillary bones, and (2) the very large molar and premolar teeth. (Proceeds. Biol. Soc. Washington, Vol. X, 1896.)

ENTOMOLOGY.[1]

Professor Forbes' Eighth Report.—The nineteenth report from the office of the State Entomologist of Illinois, covering the years 1893–4, has recently been issued. It is the eight report of the present incumbent, Professor S. A. Forbes, and adheres closely to the lines of thorough and accurate record, which have made its seven predecessors notable in the literature of economic entomology. The bulk

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

of the volume (189 pages) is devoted to the Chinch Bug—the arch-enemy of Illinois agriculture, a voluminous record being made of the experiments with contagious diseases carried on by the entomologist and his assistants. There is also an article on the White Ant in Illinois, and in an appendix of 65 pages Mr. W. G. Johnson, assistant entomologist, gives an excellent discussion of the Mediterranean Flour Moth.

Flies Riding on Beetle's Back.—Rev. A. E. Eaton, the well-known British entomologist, writing from Bône, Algeria, sends this interesting note to the *Entomologist's Monthly Magazine*: "Across the mouth of the Seybouse, on sandy pasture land bordering the seashore, big coprophagous beetles are common, sheltering in large holes in the soil when at rest, and running about on business. A small species of *Borborinæ* may often be seen riding on their backs, chiefly on the pronotum, and about the bases of the elytra—sometimes half a dozen females on one beetle. The beetles occasionally throw themselves on their backs to try and get rid of them by rolling; but the flies elude all their efforts to dislodge them, dodging out of harm's way into the joinings of the thorax and out again, and darting from back to breast and back again, in a way that drives the beetle nearly mad. In vain she scrapes over them with her legs; in vain does she roll over or delve down amongst the roots of the herbage; the flies are as active as monkeys, and there is no shaking them off. It is difficult to get them off into the killing bottle; nothing persuades them to fly; and they would very much rather stick to the beetle than be driven off it down into the tube."

Proteid Digesting Saliva in Insect Larvæ.—Dr. Wilibald Nagel describes[2] the method of feeding in larvæ of *Dytiscus*. In these larvæ the mouth is very much reduced in size, and the ingestion of food is performed by means of suction through the much modified mandibles, the process being facilitated by the powerful digestive action of the saliva. Under natural conditions the larvæ eat only living animals, but in captivity they will also take pieces of meat. The saliva has a marked poisonous action, killing other insects, and even tadpoles of twice the size of attacking larvæ, very rapidly. The larvæ not only suck the blood of their victims, but absorb the proteid substances. Drops of salivary juice seem to paralize the victim and to ferment the proteids. The secretion is neutral, the digestion tryptic. Similar extra-oral digestion seems to occur in larvæ of ant-lions, etc., and

[2] Biol. Centralbl., XVI, 1896, 51–57, 103–112.

spiders, and according to Krause, in Cephalopods.—*Journ. Royal Micros. Society.*

Weismann on Dimorphism in Butterflies.—For some time *The Entomologist* has been publishing a series of interesting articles by Dr. August Weismann on the Seasonal Dimorphism of Lepidoptera. The June number contains a recapitulation from which we take this extract: "Although I am far from considering the few experiments, which I could here put forward, as sufficient for reaching a decisive settlement of our opinions on seasonal dimorphism, yet I cannot forbear arranging them, provisionally at least, in reference to our general conceptions of the subject. When, in the year 1875, I first set about investigating the ways of this striking and yet so long neglected phenomenon, I assumed that it was to a certain extent obvious, that this kind of dimorphism was everywhere a direct result of the various direct influences of climate, principally of the temperature, as it effects in regular alternation the spring and the summer brood of many-brooded species. I had also well considered the other possibility, that dimorphism connected with the time of the year might also depend upon the *indirect* influence of the changing environment, *i. e.*, that it might depend upon the *adaptation* to the varying environment of the butterfly according to the time of year."

I then said: "It is not inconceivable in itself, that phenomena occur among the Lepidoptera analogous to the winter and summer clothing of Alpine and Arctic mammalia and birds, only with the difference, that the change in coloring does not arise in one and the same generation, but alternately in different ones." But, at that time the fact that the *upper side* of butterflies, which is usually not adaptive, can be very variable just in summer and spring, sometimes more so than the adaptive under side, appeared to me to contradict this adaptation of seasonal dimorphism. Yet, it was the fact, that the one or the other seasonal form could be produced artificially by the operation of a higher or lower temperature, *i. e.* the stamp of the winter form might be impressed on the summer brood, and *vice versa*. I therefore concluded that it was the measure of heat which was acting during the pupal period which directly formed the species in one way or the other; and I felt the more justified in so doing, as the climatic varieties form a parallel to the seasonal forms, and as the former must, without doubt, be referred to the direct influence of climate, especially of temperature.

Thus, for example, *Chrysophanus phlœas* is seasonably dimorphic in Sardinia and at Naples; the summer form, which develops during the

summer heat, is very dark, almost black, but the spring form corresponds with our German red-golden *phlæas.*

Although to-day I still look upon this view as correct, and a directly altering effect of temperature as proved, yet I have gradually been convinced, that this is not the sole origin of seasonally dimorphic varia-bllity, but that there is also *adaptive seasonal dimorphism.* We must, I believe, distinguish *direct* and *adaptive* seasonal dimorphism ; and, I see in this distinction an important advance, which, before all, places us in position to explain the results of the various experiments undertaken by myself and others in a much more satisfactory manner.

I have already pronounced this view in a lecture delivered at Oxford in the beginning of 1894, and I have sought to show that adaptive seasonal dimorphism, which I had previously only put forward as possible, does *actually* occur. The example there given for perfect insects was, indeed, only a hypothetical one, *viz.*, the case of *Vanessa prorsa-levana ;* but for larvæ, at least, I can select an example from Edward's excellent work on the North American butterflies with tolerable certainty, *viz.*, that of *Lycæna pseudargiolus,* which will be more accurately discussed later on. I did not then know what I learnt shortly afterwards from an interesting little pamphlet of Dr. G. Brandes, that cases of seasonal dimorphism had been known for a long time among *tropical* butterflies, and that among these, at least, one of the seasonal forms depend upon the assumption of a special protective coloring. Brandes maintains, with justice, that the view hitherto widely held among us is erroneous, according to which seasonal dimorphism was not to be expected in trophical countries, since the alternation of seasons is absent there. Periods of rain and drought, at least for many tropical countries, form such an alternation very sharply. At any rate, Doherty, and, somewhat later, de Nicéville, have pointed out, for Indian butterflies, a series of seasonally dimorphic species, not merely by the observation of the alternation of the two forms in nature, but by rearing the one form from the eggs of the other ; thus among Satyridæe of the genera *Yphthima, Mycalesis,* and *Melanitis,* and for the species of *Junonia,* it is accepted as proved ; and in all these cases the difference between the two forms principally consists in the fact that the one form seems like a dry leaf on the under side, while the other possesses another marking, and at the same time a number of ocelli.

Without engaging in the controversy as to the biological value of these ocelli, I do not for a moment doubt but that the coloring with ocelli is also an adaptive form, possibly protective or intimidating coloring. If one of the two forms had no biological significance, it could

no longer exist; the single adaptive one would have replaced it. But it is obvious that the appearance of complicated details of marking and color, such as ocelli are, cannot be simply the direct effect of heat or cold, drought or humidity. *These influences are not the actual causes of such formations, but only the stimulus, which sets their primary constituents free, i. e.*, induces their development, as I tried to demonstrate in the lecture above noted. As the sufficient cause of the sleep of the marmots does not lie in the cold, but in the organization of the animal which is adapted to the cold, and as the cold only brings the existing predisposition to winter sleep into play, so among these butterflies with adaptive seasonal dimorphism the display of the one or the other marking is apparently connected, partially, at least, with one of the above named outward influences, although in reference to these trophical butterflies we do not yet know to which of them.

We recognize *temperature as the stimulus to development* with the cases of seasonal dimorphism of our indigenous butterflies, as in all cases of seasonal dimorphism, which have hitherto proved experimentally, it is always high and low temperature which gives the outward impulse to the appearance of the one or the other form where this impulse did not come exclusively from *within.*

There are, therefore, two different sources of the appearance of seasonal dimorphism: on the one hand, the *direct action* of alternating external influences, *viz.:* temperature, can bring about this change in the outward appearance; and on the other hand, the processes of selection. It is therefore necessary to consider these two kinds of seasonal dimorphism separately. It will certainly not always be easy to decide between them when a particular case has to be dealt with, as at present it is not always possible to say whether a coloring or marking has a definite biological value or not. Both causes also may co-operate in in one species.

Note on the Classification of Diplopoda.—The admitted impossibility of formulating a generally satisfactory definition of the term species exists partly because systematists have used it in the greatest variety of applications, and partly because natural groups are so diverse in structure and developmental history that a scheme calculated to elucidate one may increase confusion in another. It is hence desirable in proposing or making use of a classification to recognize as clearly as possible the conceptions under which the arrangement into the various categories of natural groups has been made.

The structure and distribution of the Diplopoda make it advantageous and usually easy to arrange them into species, which are groups

of very similar individuals not connected by intermediate individuals with other groups different in details of structure, form or color. An apparent and probably sufficient cause for this is the close similarity of all Diplopoda in life-histories, habits and food. All are scavengers, able to subsist upon a variety of decaying vegetable, or even animal matter, and there has been scarcely any response to calls for special adaptations to life as parasites, commensals, or under other changed conditions. The species of Diplopoda are not only extremely local in distribution, but are generally confined to almost identical habitats, removed from which they do not long survive.

Supposing the Diplopoda to be a natural group descended from a common ancestor, we are compelled to believe that such differences as appear among them are the result of accumulated variation not greatly influenced by external selective causes. Hence, existing differences indicate in general much more remote developmental divergence than in groups which have entered more thoroughly into the struggle for existence by responding to the demands of varied conditions. In this respect the Diplopoda offer a most striking contrast to the Hexapoda, and the results are in accordance; there are more millions of species of Hexapoda than there are thousands of Diplopoda.

Having accepted a criterion of species, the classification into higher groups is perhaps largely a matter of convenience; but convenience, scientific accuracy, and the recognition of affinities, alike demand constant attention to the fact, that the value of any character depends primarily upon its constancy, not upon the apparent *degree* of divergence. This is merely the reiteration of the chief axiom of systematic science, but the abundance of systems which completely ignore this fundamental idea are evidence that much reiteration is still desirable.

While in some natural groups it seems necessary to recognize subdivisions not definable by any constant character or complex of characters in the Diplopoda, we may conveniently proceed upon somewhat better ground, and require that the genera and larger divisions shall be limited by definite structural characters.

A dichotomous classification is theoretically the only exact one, for the reason that three or more natural groups could never be expected to be separated by exactly equivalent structural differences. Practically, however, a dichotomous system is inconvenient by reason of the great number of categories necessary in properly recognizing affinities. Hence, it is not a valid objection to the usual or multifid form of classification that the natural divisions arranged under the same category are not of the same rank, that is, not remote from

each other by equal structural distances. All that can be reasonably demanded of a classification is that its groups of all ranks shall be natural ones, and that the higher the groups, the more constant, and hence fundamental, shall be the characters by which they are separated. Furthermore, it must never be supposed that the variability of a character in one group need affect its importance if found to be constant in another.

As a general policy it is evidently desirable that scientific names of all grades shall mean as much as possible. The objection to the recognition of distinct and definable genera and higher groups on account of the consequent multiplicity of names is usually to be taken as an unscientific willingness to ignore structural differences and natural affinities, in the hope of escaping additional labor. In reality the difficulty of defining groups containing unrelated members, and of becoming acquainted with such through descriptions, much exceeds the temporary inconvenience resulting from change of names.

In attempting to embody in the classification of the Diplopoda a recognition of certain structural differences found to be invariable, several natural and distinct groups of families have been recognized as orders. It is here proposed to render this classification more definite and consistent by the division of two of these orders, in the belief that the resulting groups, in addition to numerous structural differences, have long been divergent in developmental history. The orders thus to be divided are the Diplocheta and the Merocheta. From the Diplocheta it is proposed to separate the true Iulidæ and their allies, under the name ZYGOCHETA, leaving under the Diplocheta Spirostreptoidea and Cambaloidea. The Zygocheta are distinct in many characters of the gnathochilarium, in the transformation of the first pair of legs of males as clasping organs, the adnate external seminal ducts, the absence of legs from the third segment, the presence of legs on the fourth segment, and the structure of the copulatory organs of both sexes. The Diplocheta have the first pair of legs nearly or quite unmodified, the external ducts distinct, the third segment with a pair of legs, and the fourth segment footless. Notwithstanding these and other important and invariable differences, it remains probable that these two orders are more related to each other than to any third group of Diplopoda.

The other case is similar; the Merocheta will, in the restricted sense, contain numerous families allied to the Polydesmidæ, with twenty closed segmental rings; the new order CŒLOCHETA will accommodate the

Lysiopetaloidea and Craspedosmatoidea,[3] and is characterized by the greater number of segments, the free pedigerous laminæ, the seven-jointed legs, the distinct mentum, and the normal presence of eyes. In the Merocheta the apertures of the external seminal ducts are small openings in the chitinous wall of the coxæ of the second legs, connecting with internal tube of nearly uniform diameter. In the Cœlocheta the coxæ contain a large cavity, while the aperture is large, the margin pilose and not chitinous.—O. F. Cook.

EMBRYOLOGY.[1]

The Tentacular Apparatus of Amphiuma.—In the Journal of Comparative Neurology, Vol. VI, March, 1896, Professor J. S. Kingsley has written an article entitled "On Three Points in the Nervous Anatomy of Amphibians" in which he has endeavored to show that the tentacular apparatus of Amphiuma, briefly described by me (Journal of Morphology, Vol. XI, No. 2), has been mistaken for a nerve and blood vessel. I consider the discovery of this degenerate organ of too much phylogenetic importance to be consigned at once to oblivion, and, therefore, offer in this article the results of a more careful study of it.

Since histological detail is important in this investigation, I state briefly the technique. The specimen, seventy-eight millimeters in length and seven millimeters in body diameter, was hardened in Kleinenberg's picro-sulphuric and, passed through the alcohol series from seventy to one hundred per cent and returned to seventy per cent, when the head was severed and placed three days in borax-carmine, then in acid alcohol twenty-four hours, after which it was imbedded in paraffine by the usual method and cut into serial sections one twenty-fifth of a millimeter in thickness.

Figure I is magnified twenty diameters. The outlines of all the features were drawn with a *Zeiss camera lucida*. Every feature appears in

[3] From the true Craspedosomatidæ there may be distinguished the Trachygonidæ, Conotylidæ, and Cleidogonidæ, in addition to the Chordeumatidæ established by C. L. Koch in 1847. The separation of other equivalent groups will probably be necessary when a fuller knowledge of European and Asiatic forms is gained.

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

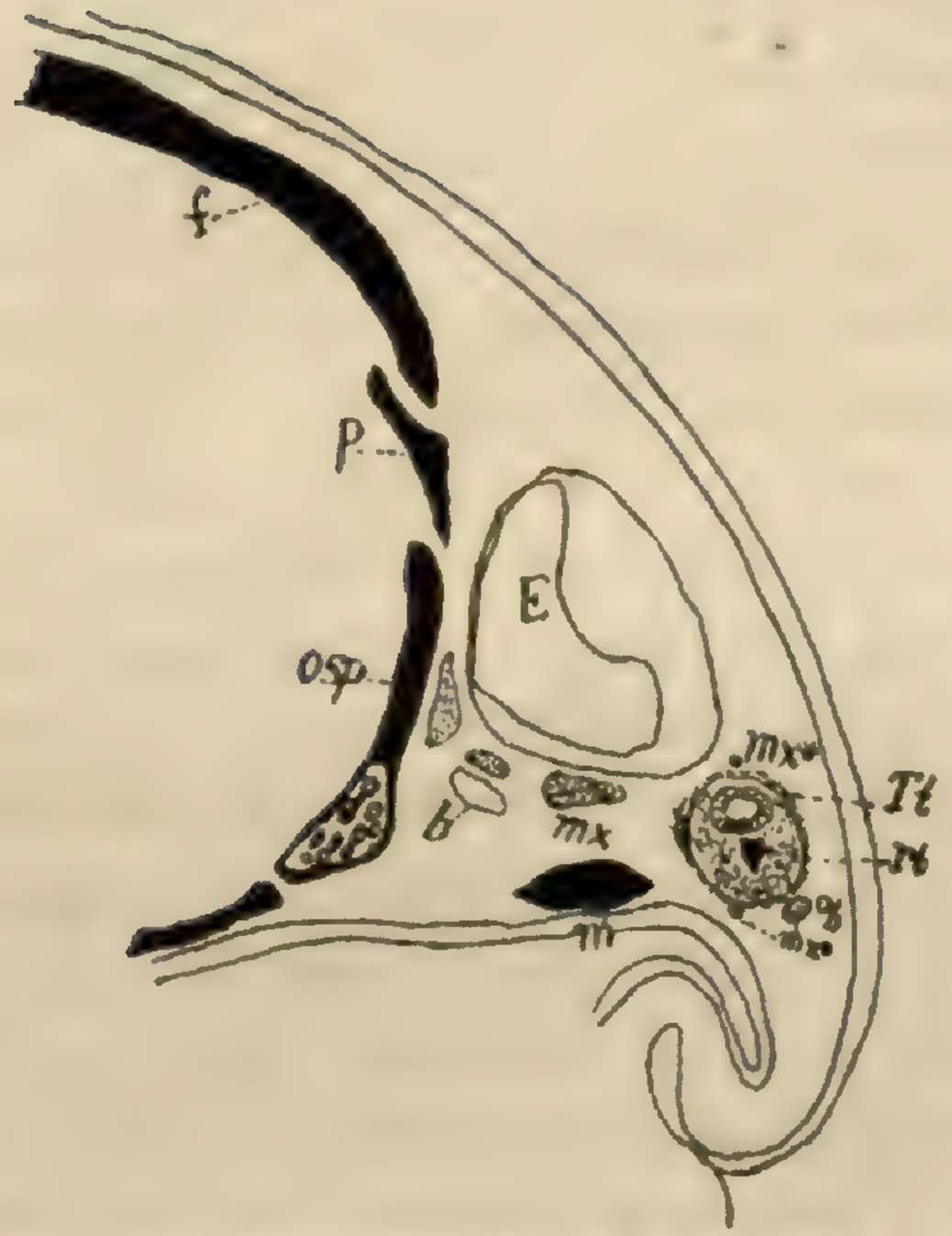

Figure I. Right-hand portion of section through head of Amphiuma 78 millimeters long, f, frontal; P, parietal; OSP, orbitosphenoid; E, eye; m, maxillary bone; mx*, branches of maxillary nerve; Tt, tentacular apparatus; rt, retractor muscle; mx, maxillary nerve.

the section just as distinctly as it is shown in the figure, b is the blood vessel and the adjacent mx* the nerve which Kingsley thought I had mistaken for the tentacular apparatus, Tt. Notice that three branches of the ramus maxillaris course along the external sheath.

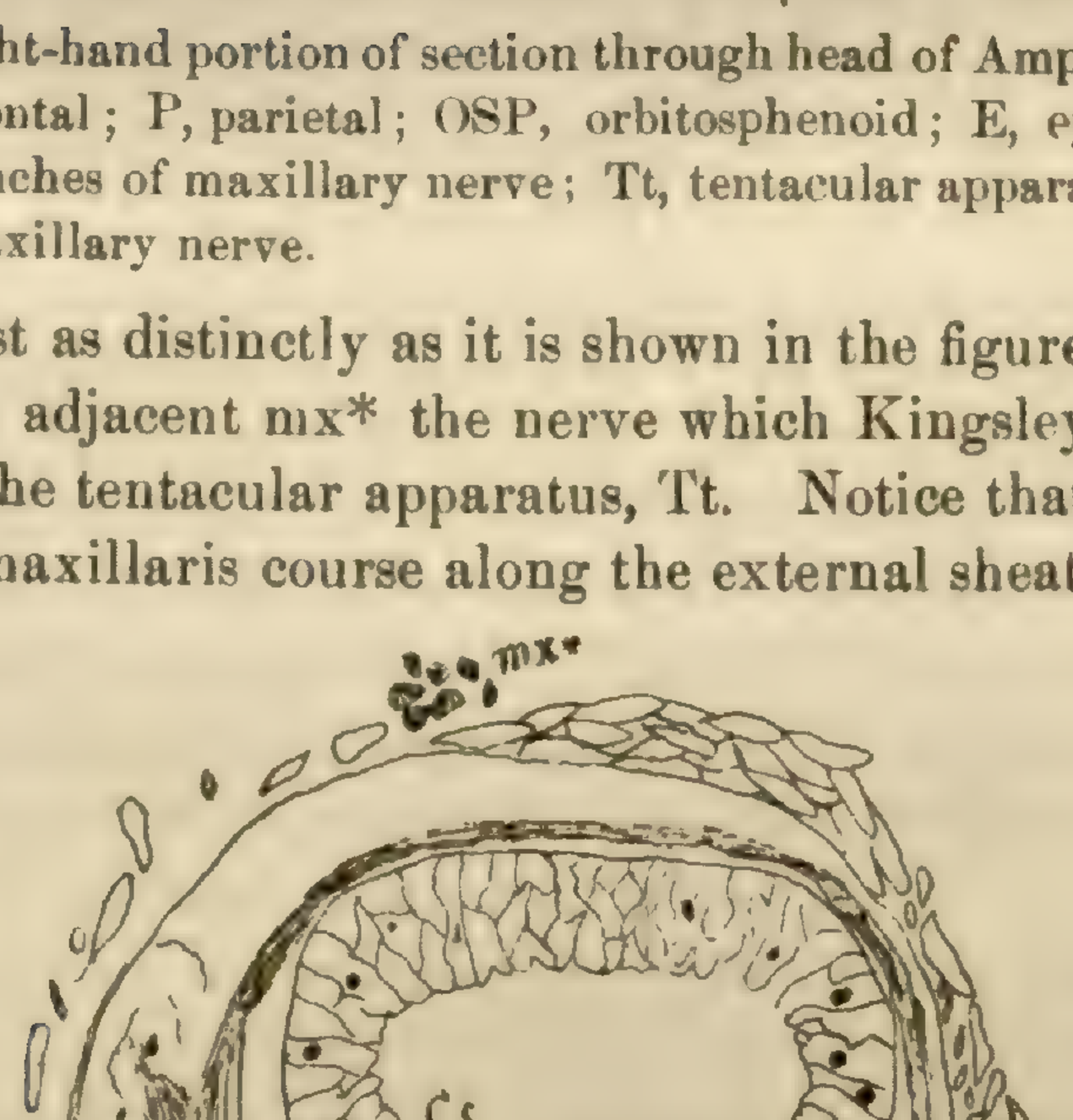

Figure II. Cc, canal for tentacle; rt, retractor muscle; ObD, orbital gland; ITts, inner sheath; ATts, outer sheath.

The histological details of the apparatus Tt. are shown in figure II as they appear viewed with a $\frac{1}{12}$ inch oil immersion lens giving about 1000 diameters. While the columnar epithelial cells lining the tentacular canal Cc are not so regular as one sees in a functional organ yet they are so well defined, especially in the lower portion that the observer cannot be misled as to their identity. The nucleus is visible in about one half the cells and the nucleolus is apparent in many cases. In the upper portion the cells have lost their nuclei and are in a degenerate condition. rt is a cross-section of a muscular element which I believe is the atrophied remains of the muscular retractor of the tentacle. In my preparation, only the bony and muscular tissues have taken on the very light shade of red which characterizes rt. Since the latter is certainly not a bone, I infer it must be a muscle, and if a muscle what other function could it have had than to retract the tentacle. This muscle is visible in ten consecutive sections while the canal Cc appears in greater or less completeness in thirteen sections. The black dots of various sizes seen irregularly distributed throughout the glandular tissue ObD may possibly be nuclei as they are stained a deep red or they may be scattered nerve fibres whose connection with the ramus maxillaris on its branches I have not been able to demonstrate because the degenerate glandular tissue was so loose as to be displaced in several sections. The irregular wavy lines, I think represent cell boundaries. These are visible with an enlargement of two hundred diameters in the lower portion but can scarcely be seen with an oil immersion immediately beneath the canal. ITts is the inner tentacle sheath composed of connective tissue fibres. It is clearly seen in eighteen consecutive sections. ATts represents the outer tentacle sheath which with a low power can be seen in twenty-five consecutive sections. Thus it is observed that this tentacular apparatus is about one millimeter long lying below and external to the eye.

The tentacular canal is complete in only four sections. Figure III represents the fourth section posterior to figure II. The columar epithelium has disappeared on the dorsal side where the inner sheath enters and on one side lies close to the wall, while on the other it mingles with a loose tissue T which may be the remains of a tentacle. This tentacle is prominent in six sections, in three of which the canal is complete so that the inner sheath does not enter it. The lumen of the canal varies but slightly in size. The musculus retractor rt dwindles as we pass anterior or posterior of the section shown in figure II. The glandular tissue decreases both anterior and posterior to the median section. The portion on the ventral side persists the longest, being present in thirteen

sections. The outer tentacle sheath retains the same circumference in about thirteen sections. As soon as the canal and glandular tissue have disappeared the circumference of the outer sheath lessens in both the six posterior sections and the six sections anterior to the thirteen

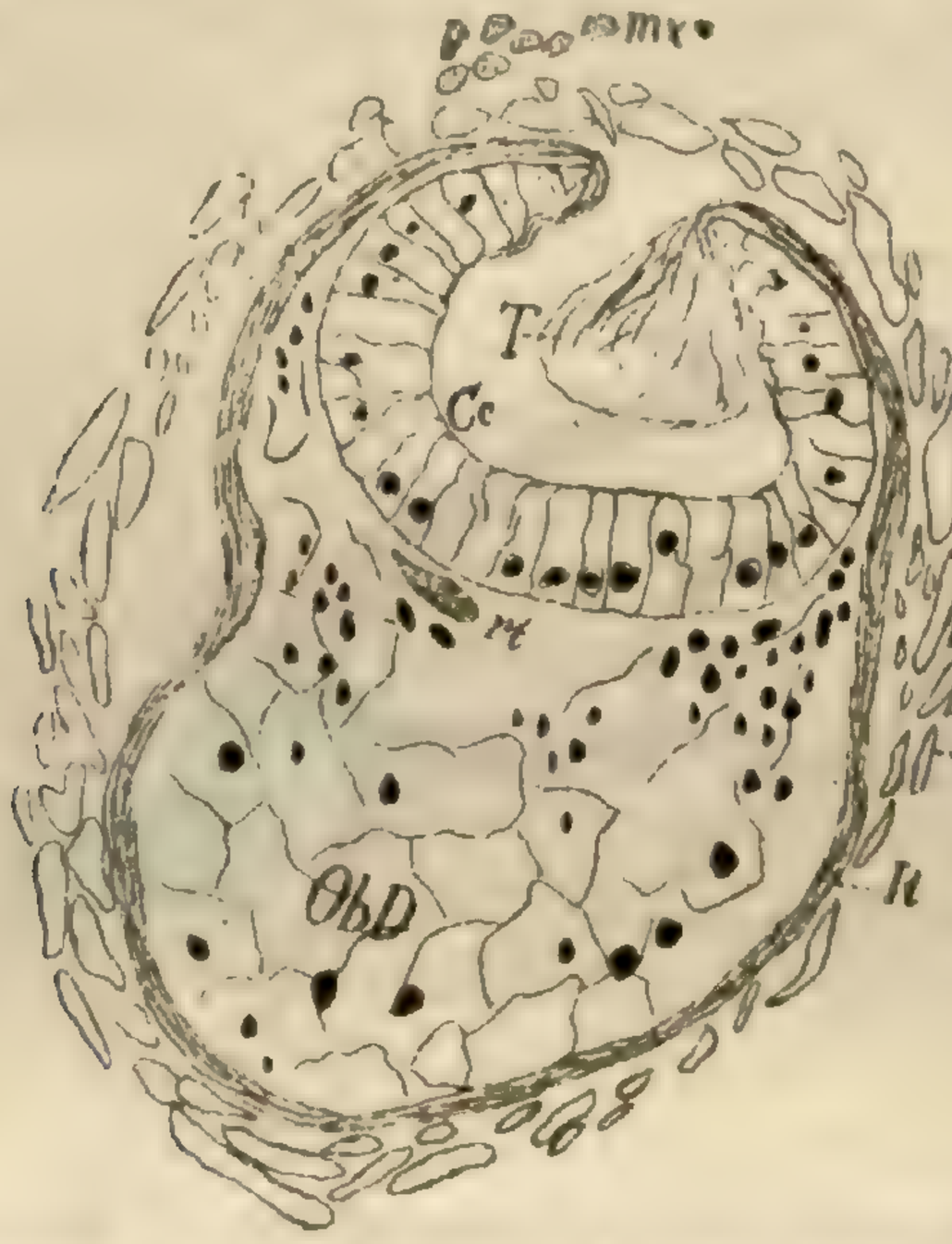

Figure III. T, tentacle; mx,* branch of ramus maxillarios; other letters same as in figure II.

median sections until it is only one fourth of the full size and the cells of the sheath become scattered, thus finally filling up the central area and creating a solid cord in the last two sections. It is worthy of notice that this tentacular apparatus was observed on the right hand side only in the specimen examined. In three other specimens of the same hatching, though they were several millimeters longer, no trace of the above described organ could be discerned. Kingsley has shown that no such organ exists in his specimens which were from the same lot as mine. An explanation of the occurrence of this organ in only one specimen may be found in the fact that it is an exceedingly transitory formation like the pronephros of the chick, which is present for only one day.

The second objection Kingsley makes to my observations, is that all the eye muscles are present in Amphiuma and the Sarasins say the retractor muscle of the tentacle is probably developed from the retractor bulbi. To this I answer that the Sarasins have not been able to demonstrate positively that the retractor muscle is developed from the retractor bulbi, and if it were true that the retractor muscle is developed from the retractor bulbi, I see no objection to the posterior part of the

retractor muscle functioning as a retractor bulbi after the anterior portion has undergone degeneration.

Kingsley further states that the described apparatus is not in the proper location to be compared to the tentacular organ of the Gymnophiona. In elucidating this point it is of service to compare figure I with figure IV taken from Die Anatomie der Gymnophionen von Wiedersheim.

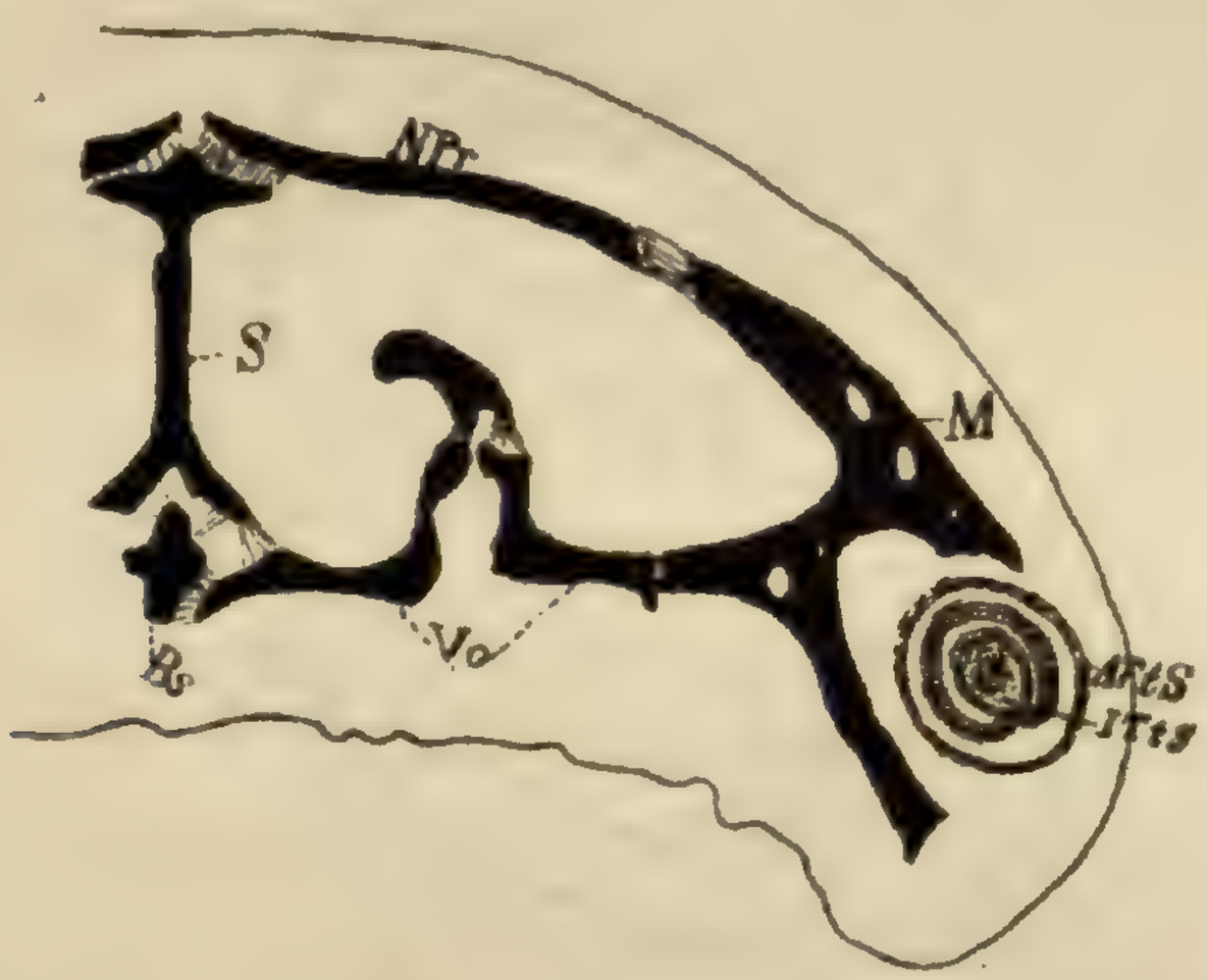

Figure IV. Cross section of *Siphonops annulatus.* NPr, naso premaxillary; Vo. vomer; M, maxillary; Atts, outer tentacle sheath; ITts, inner tentacle sheath. After Weidersheim.

It is seen that the columar-lined canal, inner tentacle sheath and outer tentacle sheath in Siphonops, have the same relation as in Amphiuma. It is further seen that the inner sheath of Siphonops is involuted ventrally to surround the tentacle while in Amphiuma a similar involution is seen on the dorsal side in Fig. III. In both genera the organ is covered merely by the skin and its subjacent tissue. The glandular tissue is not shown in Fig. IV as the section is anterior to the orbital gland. It is true the maxillary bone overhangs the apparatus in Siphonops whereas such is not the case in Amphiuma. In behalf of this contrast I quote from Cope (Bulletin of the United States National Museum, No. 34, p. 214): "There is also a very large foramen or canal passing through the o. maxillare from near its middle to the orbit, foreshadowing the canalis tentaculiferus of the cœcilia." Fig. I. is a section posterior to where the canal would enter the maxillary bone. Among the Gymnophiona there is considerable variation as to the relation of the apparatus to the maxillary bone as the following from Wiedersheim, p. 47 shows: "Sprengt man nun zum Behuf klarerer Einsicht die Deckknochen auf der betreffenden Schädelhafte volkommen ab, so wird man ein weissliches, walzenförmiges Organ gewahr, wel-

ches, wei bei Cœcilia, ganz vom Maxillarbein oder wei bei Epicrium und Siphonops an seiner äusseren circumferenz nur von der äusseren Haut bedeckt ist." Thus it is seen that the location of the organ in Amphiuma is very similar to its location in Gymnophiona.

A further corroboration of my views is noticed in the relation of the branches of the ramus maxillaris to the external sheath of the tentacle. According to Wiedersheim, in the Gymnophiona three branches of the maxillary nerve attend the tentacular apparatus in its course in the sub-orbital region. In Amphiuma I have found these three branches occupying the same relative position as is indicated by mx* in Fig. I. This striking similarity is seen at a glance by comparing fig. 54 in Wiedersheim's Anatomie der Gymnophionen with Fig. I. Before one can be convinced that the so-called tentacular apparatus in Amphiuma is really such I am aware my investigations must be verified by the discovery of this atrophied organ in other specimens. The importance of the discovery of such a feature is emphasized by Kingsley: "Were it true that Amphiuma possesses, either in the young or the adult, rudiments of a tentacular apparatus, the fact would prove of great value to those who would recognize in the Gymnophiona only degenerate Amphiumæ." Cope and the Sarasins have deduced considerable evidence favoring the close relationship of Amphiumidæ and Cœciliidæ, which fact renders it the more credible that a rudimentary tentacular apparatus has really been found in Amphiuma.—Alvin Davison, Ph. D.

PSYCHOLOGY.

Synæsthesia and Synopsia.—Until quite recently synæsthesia was regarded by psychologists generally as a purely artificial and fanciful association, or at best as a sign of degeneracy; it has lately received considerable attention, however, and the weight of evidence goes to show that it is both natural and normal—it may even be said, a phenomenon of common occurrence.

In an exhaustive monograph on the subject, published in 1893,[1] Prof. Flournoy of Geneva for the first time introduced a terminology which aimed to distinguish scientifically between the different forms of synæsthesia. The most important phase is the association of visual images, or *synopsia*. Attention was first called to this by Fechner, in 1876.

[1] Les phénomènes de la synopsie (audition colorée); by Th. Flournoy; Paris, 1893; pp. 259.

Flournoy distinguishes here between photisms, diagrams and personification. The first of these is the *audition colorée* of earlier writers; it consists in the natural association of a color with each particular sound, so that a spoken word appears to the hearer to be tinged with one or more hues, corresponding to its constituent vowel sounds. A diagram is a visual scheme in which some natural series of ideas (such as the months, days of the week, numbers, etc), is arranged. When a member of the series is recalled, the appropriate part of the diagram is visualized. Personification is simply the attributing of some personal characteristic, such as sex, to a number, etc.; or the association with it of a feeling of like or dislike. Flournoy reports some 350 persons as possessing synopsia in one or other of its forms, out of 2600 to whom questions were addressed, (13 per cent.); but as a large portion of his question-sheets were never returned, the real percentage may be regarded as somewhat greater.

In a recent paper,[2] Miss Calkins gives the results of a personal canvass of Wellesley students in 1893 and 1894. For the former year the affirmative answers numbered 33 per cent., for the latter 60 per cent. It may be doubted whether all the latter are true cases of synopsia. Yet when due allowance is made for possible temporary associations, it must still be admitted that synopsia is by no means a rare phenomenon.

Richard Hennig[3] gives an interesting study of the diagram-forms occurring in himself and his immediate family. He is able in a number of cases to trace their origin to certain associations of early childhood, and favors the 'natural,' or experiential view of the origin of all such schemes. He strongly opposes the notion of inherited forms or photisms: Only two pair in the list given by Galton, he thinks, show any real resemblance, and these may well be accounted for by similarity of early environment. "Only the tendency to synopsia can be inherited; but here the influence of heredity is unmistakable and undoubted." The writer points out a similarity between the number-form of himself and one brother brought up under the same surroundings, while in the case of another brother, whose early life was spent in another environment, the diagram was radically different. Herr Hennig urges the usefulness of the number-form as a mnemonic aid, and cites the case of a friend, who easily memorized dates by association with the appropriate places in his number-diagram.

J. Philippe has lately investigated the synopsia of blind persons, and finds a remarkable number of cases among them, though none occurred among those who were blind from birth.

[2] Synæsthesia, by Mary W. Calkins; Amer. Journalof Psychology, VII, 90–107.
[3] Ztschr. f. Psychol., X, 183–222.

With the reduction of synæsthesia to a scientific basis, which Flournoy has brought about, and the demonstration of its wide-spread occurrence, comes the demand for a more thorough examination of its bearing upon other departments of psychology. The physiological interpretation of synopsia is still unsettled, and is commended to physiological psychologists as a fruitful theme for investigation.—H. C. WARREN.

ANTHROPOLOGY.[1]

Exploration by the University of Pennsylvania in West Florida.—Little more than a year ago my friend Lieutenant Colonel C. D. Durnford formerly of the English Army, returning northward from a journey in the West Indies and Florida brought with him the specimens of aboriginal rope and netting found in a mud bed near Marco, Florida described by him in the AMERICAN NATURALIST for November, 1895.

That he realized the importance of the digging done in the mud in April, 1895 by himself and Mr. Charles Wilkins of Rochester, New York, was shown by the fact that on reaching Philadelphia he made the effort at once to present the details of the discovery to archæologists. As an original observer, a gatherer of inspiration from nature, coming generously to present us with unprecedented specimens and archæological data of much value, discribing to myself and others the details of the discovery and stating his belief that the lagoon fringing islands near Marco were net-worked with artificial canals, and would disclose other and similar relic preserving mud deposits, to him belongs the honor of opening a new door for archæology in the southeast.

The prompt recognition of the originality and value of this intelligence by Dr. William Pepper and his energetic action in cooperation with Mr. Stewart Culin, Director of the Department of Archæology have resulted in the recent expedition of the University of Pennsylvania sent by Dr. Pepper to Florida in the late months, under the direction of Mr. Frank Hamilton Cushing, whose fortunate presence in Philadelphia at the time of Colonel Durnford's visit ended in his employment by Dr. Pepper as Conductor of the Exploration. This led to the association of the Bureau of Ethnology of Washington of which

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

Mr. Cushing is a member, with the work whose results have delighted the friends of the University.

Summarized by Mr. Cushing in two newspapers (Philadelphia Times and New York Journal Sunday, June 21st, 1896) these results are represented by the array of specimens now in the Pepper Laboratory at Philadelphia. They witness the good fortune of Dr. Pepper and the University and the successful excavation of Mr. Cushing. The muck-filled artificial shell basin at or near where Coloned Durnford had worked, dammed, baled and cleaned out, and a large mound excavated 200 miles to the northward procured a superabundance of beautiful and unique remains.

The work shows that a storehouse of aboriginal manufactures escaping the notice of a good deal of reconnaissance, had lain unobserved within easy reach of scientific institutions in the east, testifying further to the fact that mud or permanent damp has here done for the Archæologist what permanent dryness has done at the Cliff Dwellings of Arizona and in Egypt. As at the Swiss Lake Dwellings here again, a whole category of remains that have perished elsewhere in the eastern United States have survived hermetically sealed in the ooze.

A few of the salient features of the collection concern:

(1) Facts relating to burial; crania from the mound and muck with funeral paraphrenalia.

(2) The relation of pottery, found in great abundance, to burial, and the allegoric and religious significance of fictile designs.

(3) The use of totemic ornaments, of masks representing the human face in ceremonials, and the allegorical significance of carvings representing the heads of animals, and paintings on wood.

(4) The economic facts of daily life illustrated by means of well pre-preserved utensils and vessels of wood and by the haftings of wood and shell implements.

(6) Interesting data refeiring to the arrangement of canals, shell walls, basins, the height of shell mounds and what appear to be vestiges of pile-built houses sunken in mud and sufficiently indicated for study.

It will not be easy for the archæologist suddenly confronted by this display of aboriginal handiwork outshining the long toiled for gatherings of other searchers in the East, to hold fast to the caution that the occasion demands, to realize how much and how little such preservation of perishable remains signifies in a given case, to remember in the inferred estimate of cultural status that multitudes of similar objects, betokening the life history of other tribes in the eastern United States have perished, in short to weigh considerations that must temper the use of

colored words signifying degree of ethnic importance, advanced methods of construction, superiority in the arts, and kinship to other peoples.

Meanwhile the excavation and production of the strange carvings in wood, the human masks, the unique paintings, the hafts of wood and tools of shell, the relics of rope and fabric, remain in evidence to speak in manifold praise of the enthusiastic searcher who while telling his glowing story has shown that he has known where to dig and dug with effect.

Symbols inscribed upon the drawings of birds, totemic buttons arrangements for burial with reference to the "four quarters of the world," the paraphrenalia of priests buried together in the mud here seek explanation at the hands of an interpreter, whose experience should have qualified him for the task. Luckily the elucidation of the allegorical meaning of the serpent and the raccoon, the gopher and the bat, the badger and the cormorant, tokens of gods of the dead and the living of the morn and the dusk, has fallen to the lot of one whose knowledge of the mystic inner life of the Indian, gathered upon a painful path of Zuni initiation might best recognize in the manifold characters of these remains a symbolism hidden to other eyes.—HENRY C. MERCER.

SCIENTIFIC NEWS.

The International Geological Congress will hold its seventh session, in 1897, at St. Petersburg, Russia. The presidential chair on that occasion will be occupied by M. A. Karpinsky. A number of interesting excursions have been planned to take place both before and after the meeting. It is proposed to visit Finland and the Ural country, to examine the basins of the Don, the Volga and the Dneiper. While the grand tour at the close of the Session covers the ground from St. Petersburg to the Caucasus, giving opportunity for special examinatlon of many interesting localities.

The circular of announcement gives the following information in the closing paragraph.

"The Committee on Organization takes pleasure in making known to you that His Majesty the Emperor, upon the report of his Excellency the Minister of Ways of Communication (Transportation) has deigned to grant to all the geologists (who give notice in time of their

intention to take part in the work of the Congress) tickets allowing them free first class transportation on all Russian railways before and after the Meeting of the Congress, including the excursions."

Lord Lilford, the President of the British Ornithologists' Union, died June 17, 1896. At the time of his death he was engaged in a work on the Birds of the British Islands which was nearly completed. He was a contributor to Ibis, The Zoologist and the Proceedings of the London Zoological Society. His interest in natural history led to his keeping an extensive collection of living animals at his country seat in Northhamptonshire.

Mr. T. D. A. Cockerell, Las Cruces, New Mexico, will be glad to furnish information concerning the biological station he proposes to establish in New Mexico. If a sufficient number of students are enrolled, a beginning will be made this summer. For the study of nsect life New Mexico presents an unusual combination of advantages.

The prizes awarded by the London Geological Society have been distributed as follows: The Wallaston Medal to Dr. Edward Suess, Ph. D. Prof. of Geology in the University of Vienna; Wollaston Donation Fund to Alfred Harker, M. A. of the Geological Survey of Scotland; The Murchison Medal to T. Mellard Reade, Esq.; Murchison Geological Fund to Philip Lake, Esq.; The Lyell Medal to Arthur Smith Woodward, Esq.; Lyell Geological Fund to Dr. Wm. Fraser Hume, Demonstrator of Geology in the Royal College of Science and Charles W. Andrews, Esq.; The Barlow-Jameson Fund to Joseph Wright, Esq. and Mr. John Storrie of Cardiff. (Quart. Journ. Geol. Soc. London, 1896.

Messrs. Hatcher and Peterson have gone to Patagonia to collect fossil vertebrata in the Cenozoic beds of Patagonia for Princeton University.

Macmillann & Co. have made arrangements for the issue in New York and London of a "Dictionary of Philosophy and Psychology" under the editorial supervision of Professor Baldwin of Princeton University.

The following assignments of topics with the names of the authorities who will contribute original matter may be already announced:

General Philosophy and Metaphysics.—Prof. Andrew Seth, Edinburgh University; Prof. John Dewey, Chicago University. *History of Philosophy.*—Prof. Josiah Royce, Harvard University. *Logic.*—Prof. R. Adamson, Glasgow University. *Ethics.*—Prof. W. R. Sorley, Aber-

deen University. *Psychology.*—Prof. J. Mck. Cattell, Columbia University; G. F. Stout, W. E. Johnson, Cambridge University; Prof. E. B. Titchener, Cornell University; The Editor, Princeton University *Mental Pathology and Anthropology.*—Prof. Joseph Jastrow, Wisconsin University. *Biology.*—Prof. Lloyd Morgan, University College, Bristol. *Bibliography.*—Dr. Benjamin Rann, Harvard University.

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Dr. Joseph F. James begs to inform his friends and correspondents that he has removed from Washington, D. C., and that after May 10, 1896, his address will be Hingham, Mass.

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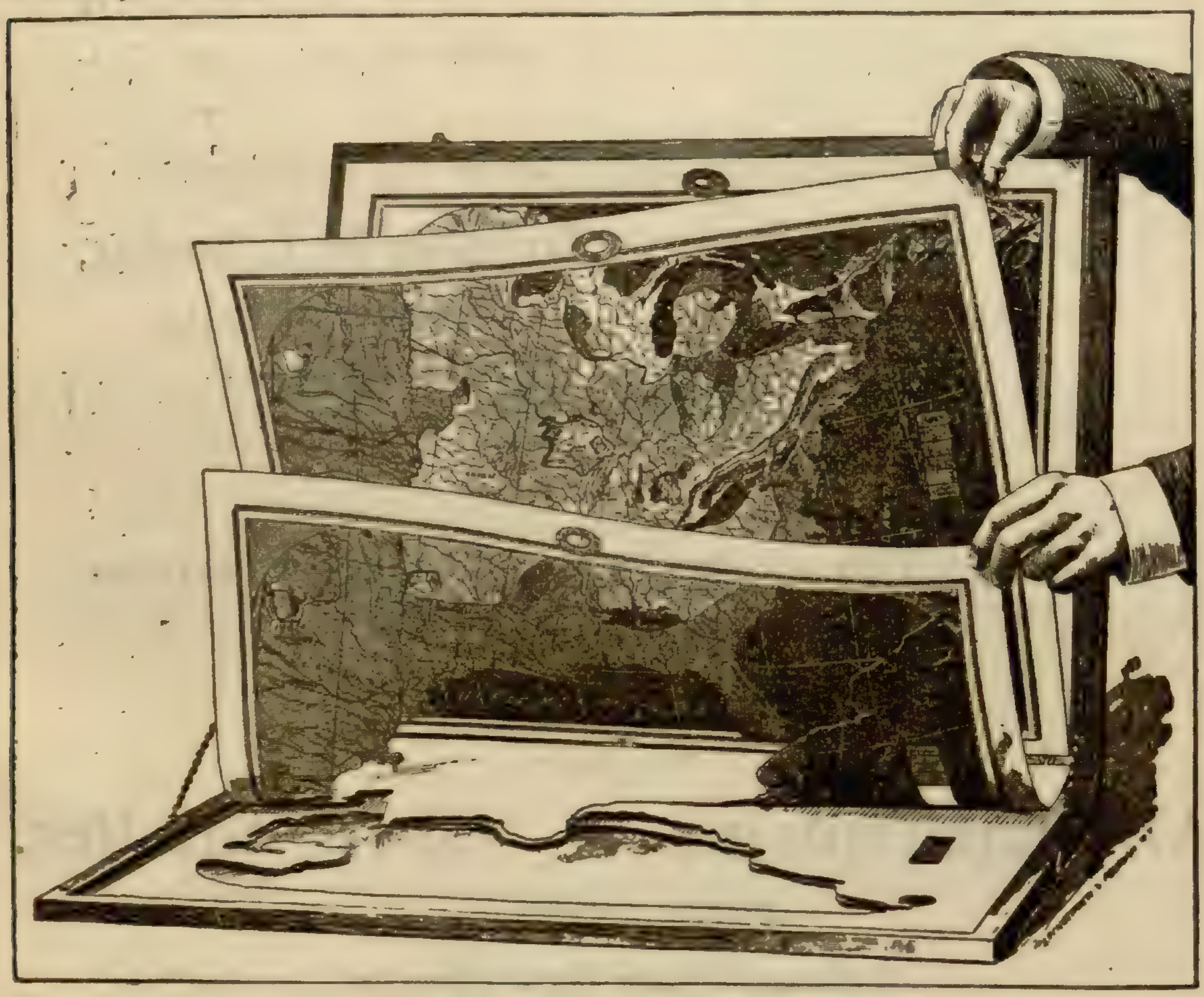

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Dr. D. G. Brinton, Rev. Wm. M Beauchamp, Prof. A. F. Chamberlain, Mr. James Deans, G. O. Dorsey, Dr. J. Walter Fewkes, H. C. Mercer, Mrs. Zelia Nuttall, C. Staniland Wake, Dr. Wm. Wallace Tooker, Dr. Cyrus Thomas. The Magazine during '95 will embrace different departments, and the following gentlemen will have charge and report all explorations and discoveries:

Rev. Wm. C. Winslow, D. D., L. L. D., Egypt.
Prof. T. F. Wright, Explorations in Palestine.
Henry W. Haynes, Paleolithics and European Archaeology.
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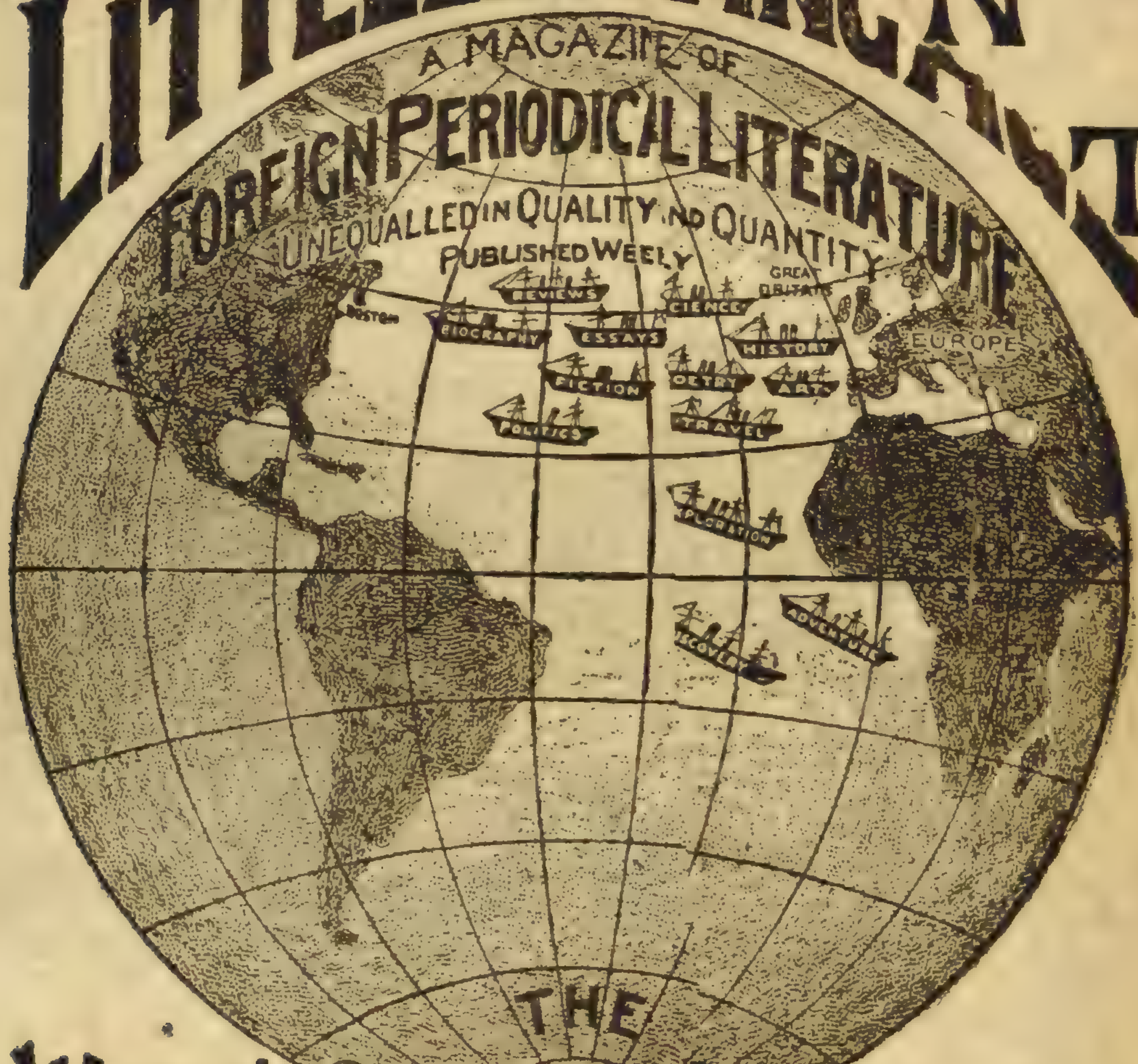

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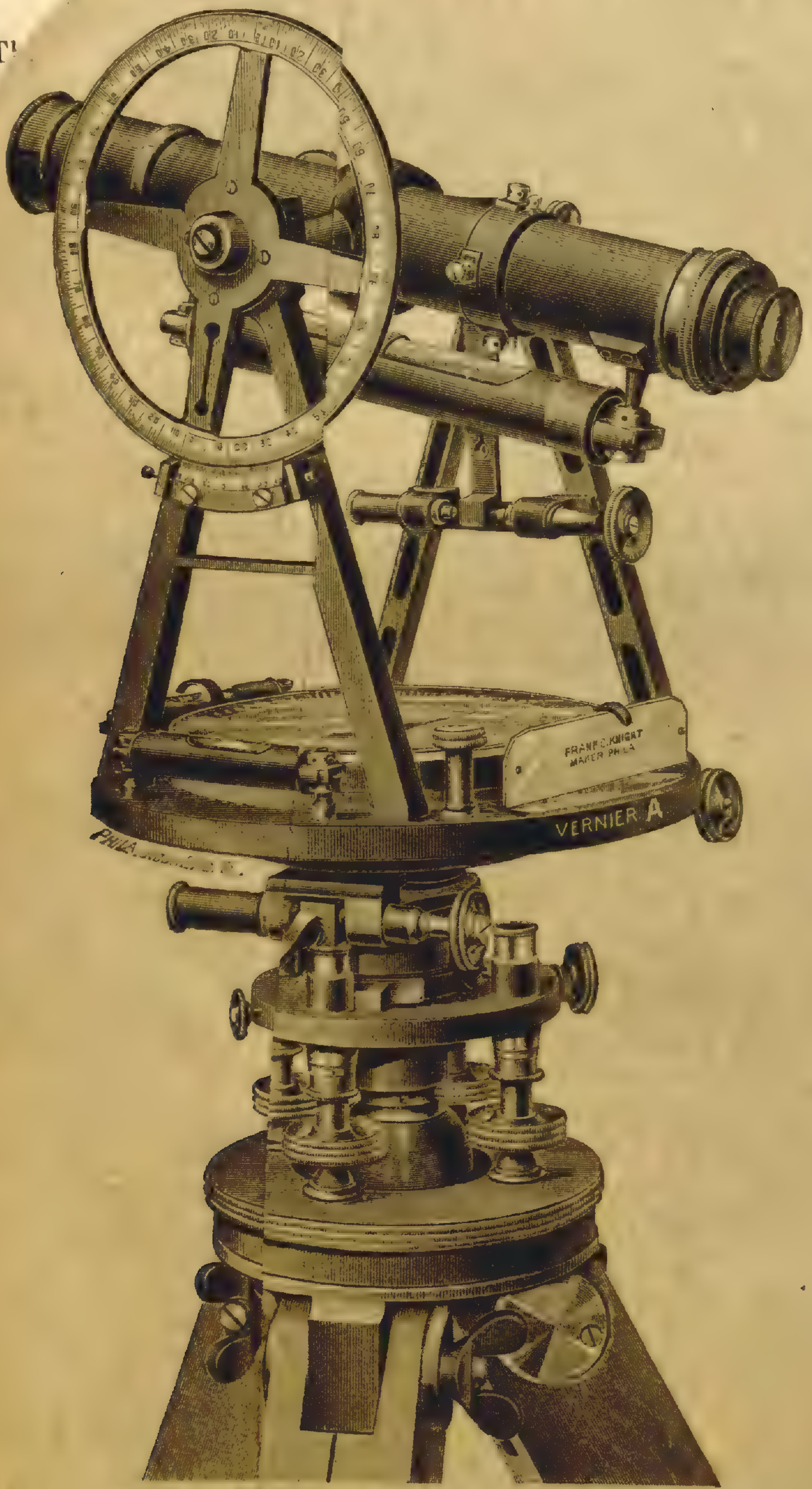

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THE
AMERICAN NATURALIST

VOL. XXX. September, 1896. 357

PROF. BALDWIN'S "NEW FACTOR IN EVOLUTION."[1]

BY HERBERT NICHOLS.

That the pendulum of opinion swung too violently against the conception that mind is an active factor in Evolution I count the major misfortune of the modern epoch of Science. That there is now a return of interest I esteem to be the most important outlook of our day. That this return of interest centres in Psychology is inevitable. If now this new movement should become abortive through any false lead of Psychology the result would be deplorable.

It is with anxiety, therefore, that I read the numerous writings of Prof. J. Mark Baldwin upon the rôle played by mind in Evolution (see above Reprint for complete list). The prolific earnestness of this author, together with his conspicuous position as professor at Princeton and Alternate Editor of The Psycological Review, give unusual prominence to his views. Yet these views, as I believe, are precisely of the kind which we have most to dread. It is in this belief that I am prompted to the analysis of them which I here propose. And as Prof. Baldwin has no more enthusiastic admirer of his sincerity and

[1] Reprinted from THE AMERICAN NATURALIST, June and July, 1896.

zeal, so I beg him to permit me to point out the more freely the objections to his main assumption.

In Professor Baldwin's latest paper, above referred to, he has "gathered into one sketch" an outline of his theory. In this pamphlet, as in all else that he has written on this subject, we are presented with a vast pyramid standing on its apex. We are told how he conceives Evolution to work under his assumption, and gradually his story narrows toward an explicit statement of what this assumption is. Unfortunately, however, the vast superstructure closes in to a cloud of mist, and does so, alas, not only before he has made clear in exact detail what his assumption is, but even before making understood how the things he vaguely suggests could ever clearly be conceived to be possible.

The gist of Mr. Baldwin's notion is that Pleasure-Pain is a psychic "factor" that crucially determines Evolution. Pleasure results from beneficial stimulus. It causes, in turn, "excessive" neural discharge. Neural discharge causes "expansion." Expansion brings the creature into continued subservience to the beneficial stimulus. Excessive neural discharge makes the paths of actual discharge more pervious to the continued stimulus and to subsequent discharges from the same source. Thus a "Circular Reaction" becomes fixed which, because it is beneficial, conduces to the preservation at once of the peculiar habit and variation in the organisms so developed, and also of the creature in which it is developed. The antithesis of all this happens with pain.

Now for the difficulties; and to bring them out let us imagine an unorganized creature before us—say an amœba. Our problem is to find how it becomes organized. Let us imagine it attacked by any given stimulus at some point of its periphery. Mr. Baldwin tell us that if this stimulus is beneficial it will give pleasure, and the pleasure will cause "excess movements." Mr. Baldwin does not pretend that these are yet organized movements. To do so would be to beg his whole question. Yet he claims that this unorganized movement would complete his "Circular Reaction" with the beneficial stimulus and perpetuate the beneficient work. *But how can we conceive*

that unorganized movement, or movement in the abstract, should do such an organized act as to select beneficial stimuli and avoid those which are detrimental? Especially how shall this be done after Mr. Baldwin has carefully laid it down than there can be no such thing as benefit or detriment in a mere muscular movement in and of itself?[2] Of course Mr. Baldwin knows that various propositions have been suggested by different physiologists to explain why an undifferentiated creature like an amoeba, puts forth pseudopodia and makes definite prehensile movements in response to certain stimuli; and makes definite revulsions in response to others. But if so he is aware that all these propositions are based upon some purely physical relationship of the different stimuli to the protoplasmic substance, whereby some act in one way and others in a reverse manner. All such movements are definite and concrete and can be perfectly understood. But how *mere movement in the abstract* should be able to select that sort of nutriment which is beneficial and to avoid those forces which are harmful is surely above human power to conceive—unless, perhaps, Mr. Baldwin can explicitly describe to us how it is to be conceived. To assume outright that the movements resulting from pleasure would locomote intelligently toward proper nutriment, or do aught differently than the same movements caused in any other way, is simply to leap the whole problem by one absolutely unbounded bald assumption. Than this it is more respectable to say that Ormozd takes the kitten by the neck and chucks it bodily to the saucer.

But, perhaps, Mr. Baldwin merely means that the excess movement would work to continue the contact with the original stimulus already made. If so, then must we contend that absolute quiescence would most conduce to the preservation of a contact already made, and incoordinate wiggling would be the thing in the world most likely to break the contact, and to drive the creature away from the beneficial stimulus.

Mr. Baldwin's assumption that excess movements, however caused, would be any more likely, in the abstract, to secure circular reactions among beneficial stimuli than among detri-

[2] Mental Development, p. 189.

mental ones is, therefore, wholly false. All would depend on the prevalence of one or the other sort of conditions. If dangers most abounded the creature would be all the more quickly destroyed by his excess locomotion. If benefits abounded then the creature would prosper because of that fact, but not because of any power of muscular tissue to select these benefits, save that be by its physical properties—i. e., the same which are being studied by the physiologists as before mentioned.

Thus falls the king-bolt in Mr. Baldwin's "circular reactions." But falling back upon the second link it does seem at first sight that advantage should be secured to a creature by a "new factor," which should have the power of saying *when* the creature should act and when not; and that had the intelligence to decide that the creature should move only when in the presence of beneficial stimuli and not move in response to detrimental ones. But here again there is a snare and delusion, and just where it was least to be expected. For it is just as likely as not that *to move* would be the most beneficial thing in the world under attack of detrimental forces—for instance, to get away from them; or that to move under beneficial conditions would be the most detrimental thing in the world—for example, would wiggle the creature away from a newly secured morsel of food. In short, so long as it remains true, as shown in our last paragraph, that abstract movement is equally likely to do harm or good, so also must it remain true, that even a "new factor," with the power attributed to it by Mr. Baldwin, could not by any possibility favor the organism by the means described. How should it by the exercise of a power which in itself is alike blind to good or ill?

Thus falls the main swivel in Mr. Baldwin's chain of reactions, and falls at a simple touch. But lest it seem to fall too easily in proportion to the mighty and world-deciding destiny asserted of it, let us pursue it further and in more detail. Thoroughly to dispose of an error we must see how and why it was made. The doctrine of pleasure, of which Mr. Baldwin's "excess discharge" is the attempted physiological expression, dates back to Aristotle. Aristotle declared that pleasure ac-

companies perfect use of our faculties, and pain their impeded use. The philosophy which prevailed after Aristotle was dominated by the Oriental superstition that the forces of this world are divided between the Powers of Good and of Evil. How this superstition seized upon and biased the dogmas of our theologic ancestors until belief in a personal Devil was universal among even the learned in the middle ages, is a matter of undisputed history. Aristotle's doctrine fitted well with this superstition, and his unquestioned authority enforced its universal acceptance. Thus, as late as 1647, we have Descartes, the highest authority of his age, declaring that "All our pleasure is nothing more than the consciousness of some one or other of our perfections." When Science dawned, and began basing mental activities upon correspondent neural processes, nothing was easier or more inevitable than that the doctrine which always had been conceded to express general conditions of welfare and activity should be transferred to general conditions of the nervous system; and that, in general, "heighted neural discharge" should be declared to be the basis of pleasure, and the reverse to be the basis of pain. Thus, an early conjecture of Aristotle, fostered by one of the grossest theological superstitions, and transformed, as I shall show, by most uncritical and fallacious physiological assumptions, is the historic origin of what Prof. Baldwin calls "A New Factor in Evolution.[3]

The origin of the notion having been accounted for independently of any critical regard of the facts, we will now examine it in the light of the facts. We have no means of examining neural discharges directly, or independently of their stimuli, their sensory effects, and their motor results; we have no other means of measuring them, except through analogy with the strength of these. In general it is fundamentally observed that where the stimulus is intense the sensation is intense. Also, muscular reaction is proportional to the stimulus and to the sensation. Every known fact, outside of the phenomina of pain and pleasure in dispute, conforms to the in-

[3] Whether it is with reference to Spencer, Bain, Descartes or Aristotle, that this factor is "new," Prof. Baldwin does not state.

ference that the stimulus, the neural discharge, the physic counterpart, and the motor result, rise and fall together. Beginning now with the motor reactions of pain, it is to be observed that they are among the strongest and most violent of which we are capable; the violent struggles that every creature makes to free himself from pain, or that he displays, reflexly, in the convulsions of its torture, are among the most familiar facts known. Again, it is equally well known, that the stimuli which cause pain are the most violent that we encounter; usually it is for that reason that they are detrimental. Also, pain is the strongest and most violent of our sensations. When, therefore, all the evidences alike, from every common source of observation, agree that the neural discharge *ought* to be strong proportionally as the stimulus, the sensation, and the motor reactions are strong, it would seem that we ought to conclude that the neural discharges of pain are strong.

Surely we ought so to conclude, unless Prof. Baldwin has further evidence to offer. The evidence most likely for him to offer is that pain is characteristic of exhaustion, weakness, disorder and disease. This is the stronghold of the traditional school, and has been the secret of its fallacy from its beginning. Yet, there is not a single one of these phenomena that is not perfectly explained without accepting the tradition, and without any of the violations of fundamental analogies which its acceptance necessitates. This is done upon the basis of specific pain-nerves. Every analogy demands that there should be such nerves. If all other sensations have specific nerves so should pain. They have long been anticipated in physiology. And recently they have been demonstrated with surprisingly wide-founded and abundant evidence;[4] quite equal indeed to that for the nerves of touch.

Necessarily the universal distribution of these nerves brings them into close connection with the vaso-motor mechanism. Wherever there is unusual congestion of the blood there is

[4] See article in Brain, p. 1, 1893, and p. 339, 1894, by Dr. Henry Head of University College Hospital, London. Also those by Prof. von Frey in Berichte d. math. phys. classe d. Königl. Sachs. Gesellschaft der Wissenschaft zu Leipzig, 1894, pp. 185 and 283; 1895, p. 166.

likely to be pain. We are not certain what the appropriate form of stimulus is for the pain-nerves, but assuming it to be mechanical pressure, then any unusual stretching or tension, whether in the capillaries or the surrounding tissues, as caused by congestion, or from undue secretion of any of the glands, or from any other disorder, would perfectly explain the attendance of pain. That this should explain the characteristic pains of exhaustion, weakness, disease, and all other abnormalities, rather than the mere loss of general bodily strength, to which the common tradition more directly attributes them, no scientist should doubt. For, first, there is no evidence that mere weakness, independently of the physiological derangements which are the co-results of *its* cause, are at all painful. A man may bleed to death, and suffer no pain. Again, a frail invalid may fade away with weakness, and suffer no trace of pain; indeed, may depart with gladness. Or a sprinter may drop with exhaustion and, perhaps, suffer no pain at all; or if any, none save what is unmistakably due to the abnormal disturbances of circulation already referred to. Secondly, all causes of weakness are likely to produce disorders which, in turn, shall produce disturbances likely to excite the pain-nerves in the way above indicated. This is so evident that it need not be discussed. Third, when so excited, even during general bodily weakness, there is still every evidence that the pain discharges are characteristically strong above other nervous activities, and relatively so proportionally to the lowering of the general level of strength. It would seem, therefore, that every known phenomena of pain, on the one hand, receives perfect explanation on the basis of pain-nerves, that every analogy demands such nerves, and that finally they have been conclusively demonstrated. And, on the other hand, it is strikingly manifest that every evidence we possess flatly contradicts the assumption that pain discharges are feeble.

The corresponding assumption that the neural discharge of pleasure is "excessive" equally fails of corroboration when confronted with the facts. Here, again, we can measure the discharge only by its psychic accompaniment, its stimulus, and its motor effect. That pleasures, among psychic states, are charac-

teristically intense, is not true. Again, that intensity of stimulus is not a uniform determinant of pleasure is one of the best known truths of every form of art. And that the motor effects of pleasure are not conspicuous for violence is not less well known. Some of them are violent, no doubt, yet abundance of others are among the most soothing and quieting influences which we experience. The entire field of pleasure therefore —source, centre, and motor discharge—is one endless contradiction of the assumption that its neural discharge is predominately intense, and points even to a new *definition* of pleasure from that of which the traditional school is possessed. Again, it is the delusive general relationship of pleasure to health, strength and welfare which has ever been the source of error. With health and freshness all functions, undoubtedly, are more vigorous, and those which give pleasure are more active among the rest. Also, in health we are freer of unpleasant disorders. Yet it remains true that the feeblest invalid is often capable of the intensest pleasure, and that the trained athlete may suffer excruciating pain if the dentist but tickle the bare nerve of his tooth with a feather.

Against the "discharge" link, pleasurable or painful, in Mr. Baldwin's "Circular Reaction," it would seem unnecessary to push the sword further. In has absolutely no foundation in fact. Yet, as this is of a class of tradition that dies hard, I will bring yet multiplied objections against it. When a child first brings its finger into contact with a flame it instinctively draws its arm away: a complicated and delicately articulated mechanism has been evolved by nature, and inherited by the child for this purpose. The case is typical, and other examples are innumerable. *Now, under Mr. Baldwin's Plan of Evolution, it would have been impossible for such an organized response to pain to have developed.* His whole scheme is one wherein "the excess discharges" of pleasure conduce to the development of organized responses to pleasure, and the "restricted discharges" of pain *specially prevent the development of organized responses to pain.* It is true that Mr. Baldwin expressly declares his "New Factor" to be ontogenic. Still, if so, then pain restrictions must have yet worked from the moment of each creatures

birth to stamp out every provision of the type above cited. Over and above this, *every intelligent organization against detrimental forces would be impossible from the moment of birth.*

This is no small obstacle to the universal acceptance of Mr. Baldwin's "New Factor," yet the more intimately we approach it the more do the difficulties increase. This time for a bull's-eye example we will take a plunge straight at the "pain-pleasure discharge" itself. Mr. Baldwin tells us it is "central"—let us now ask to what is it proportional? What gauges its "heightening" or its "restriction?" The pain or the pleasure, of course, Mr. Baldwin answers, since his "New Factor" is a psychic factor. But to which is the pain or pleasure proportionate—*the incoming sensory nerve current*, or the "*benefit from the external stimulus?*" It is just here that a "tremendous" (to use a favorite word of this enthusiastic writer) stumbling-block arises. Mr. Baldwin tells us with emphasis that the pleasure comes in and by the stimulus. But how and in what manner does the external pleasure-stimulus connect with the centrally rising "heightened discharge"? Plainly it cannot be through the mere intensity of the ordinary incoming sensory nerve-current; for the pleasure is proportional to the benefits from the external stimulus; and these benefits are by no means proportional to the intensity of the stimulus. But, perhaps, Mr. Baldwin conceives—he does not tell us here in the least what he does conceive, though it is an absolutely essential point—of some *specific kind* or *mode* of neural activity to convey his pleasure-stimulus from the periphery to the centre, and one in no way parallel to the intensity of the external stimulus. If so, then a still greater difficulty now arises to conceive how the "benefit" or the "detriment" from the external event expresses itself through this new mode of communication. We are told that the pleasure is proportional to the *amount* of the benefit worked by the stimulus, not to its intensity. But just how and when does this "amount" get transformed into this new kind of ingoing pleasure current? Benefit is a "tremendously" abstract affair. Where does it end, and when does it act? The benefit does not happen instantly—when then is its pleasure experienced? How and

when does it sum itself up with reference to the heightened motor discharge? For this last, we had supposed, resulted immediately upon the arrival of the sensory impulse at the brain, and cannot be permitted a long delay if it is to join in "Circular Reaction" with the passing stimulus.

Surely here is a puzzle! Let us endeavor to follow a concrete example; and again it shall be Mr. Baldwin's own, wherein he explicitly describes the sort of betterment that gives pleasure and "heightened discharge." When the sun shines on a creature its warmth promotes nourishment and other vegetative functions. Let us say now that it heightens digestion from a usual period of two or three hours to one of twenty minutes. *When, then, does the "central discharge" begin to be "heightened" by this betterment in order to complete Mr. Baldwin's "Circular Reaction?". Also, just how does the benefit gather itself together from the bowels to express itself as the pleasure of the original sensation; i. e., the sensation of warmth that came at the beginning of the twenty minutes?* A diagram drawn to scale of these physiological activities, and with their space and time processes accurately portrayed, would facilitate the acceptance of Mr. Baldwin's "New Factor" among scientists generally.

But, of course, all this is doing the utmost of injustice to Mr. Baldwin's "New Factor." For, is it not a psychic factor? And is it not the essence of psychic factors to surmount all lawful relations of space and intensity? How absurb of me to attempt to trace the benefits and detriments of the sun's rays through the viscera to the "heightening" and "restricting" of central discharges! Pleasure and pain, of course, are super-spacial and super-temporal fiats that leap all physical difficulties and bounds. Only why, then, does Mr. Baldwin take the trouble to localize them as *central?* Or why declare them to have any mechanical relationship with motor discharges? It is just here that I must plead it to have been most natural for me to have been mislead to conceiving that the "central" processes of pleasure have some lawful articulation with the incoming sensory impulse, since they are explicitly declared to have both temporal and spatial articulation with the outgoing motor discharges. But, perhaps, Mr. Bald-

win's vagueness and confusion of statement and *longing* to be scientific may here have got the best of these outgoing articulations—perhaps, they do not and could not work according to any known axioms of science even here! Let us examine this.

Upon close consideration it becomes obvious that Mr. Baldwin's "New Factor" not only interrupts all normal relations of intensity between incoming stimuli and outgoing discharges —so that feeble stimuli, if beneficial, now produce "heightened discharges," and violent stimuli, if detrimental, are "tempered to the shorn lamb"—but also it wholly *transforms* their mechanical effects. Not only now do pleasures produce "expansion" and pains "restrictions," but violent pleasure-discharges produce violent expansions, and violent pain-discharges produce violent contractions. Or, at least, I suppose they do; though here is the very pesky plague of it, to know what Mr. Baldwin does conceive to happen. For, if now the "discharges" do thus cause literal bodily expansion or contraction, in due accord with their intensity, it is impossible to conceive what "heightening" or "restricting" has to do with the case. And, on the other hand, if "expansion" refers to "degree of activity," and "restriction" means "quiescence," I give up trying to understand the matter, and plead insanity and hallucination at once; for then the innumerable acts which seem to be performed before my eyes, both expressively and preventatively of pain must be "restricted" absolutely, and by no possible means actually can happen; and the cause of my derangement in conceiving that they do is surely sprung from my overwrought sympathy for all physiologists or psychologists who shall attempt to measure the amount of restriction necessary to be applied to each varying intensity of incoming detrimental stimulus in order to reduce it to a constantly maintained zero of quiescence, and not have the least little bit over to set the creature wiggling right up to its detrimental persecutor, and perpetuate "Circular Reaction" therewith, just as if it were beneficial.[5]

[5] The physiological cult of the traditional Pain-Pleasure School juggle much with this "heighted and restricted central discharge," as if it were ordinary neural activity. But when all obfuscation is nailed to the board it becomes plain that, since the muscular effects come after the motor nerve-currents are formed,

But, seriously, there are a few things that we must conclude regarding Mr. Baldwin's "New Factor," if we are to pay to it any logical regard whatever: (1) The work assumed of it is not one of simple heightening and restricting, but one of absolute interruption, transformation and reversal of natural consequences. (2) These interruptions, transformations and reversals proceed by no known axioms or measurements of science, and as little so in their articulations with the motor apparatus as with the disseminated benefits and detriments from the external forces. (3) There are no central neural processes correspondent to these alleged activities of pain and pleasure. There are no facts which suggest them; no physical activities could behave in such disregard of physical laws; and to assert them as acting by such laws would either duplicate the "New Factor" as an efficient cause, or else reduce pain and pleasure to ordinary non-interfering parallelism; which is a flat contradiction to Mr. Baldwin's entire proposition. (4) It is absurb to locate this New Factor as "central." For a factor that transends all physical laws of space, time and intensity cannot be located in this physical world. (5) And, finally, if such a "New Factor" existed, any exact determination in physiology and in psychology would be futile. Whereas the psychic factor of Prof. James is a wee and comparatively inoffensive affair, which only tips a molecule here or turns a current there, just a little, and when absolutely needed—and apparently from the remainder of his system is never needed—on the contrary, this Factor of Mr. Baldwin's is the dominant

therefore it is something beside *the mere intensity* of these currents which determines whether the result shall be expansion or restriction. And if it is something different from the intensity of these nerve-currents, then also must it be different from the ordinary intensity of the central neural activity which gives rise to these currents. In which case it is nonsense to talk of "heightening and restricting" precisely as if they were performed by ordinary central activities.

Unmistakably it is no "ordinary" activity that either destroys ordinary intensity regardlessly of all opposing parallelograms of forces, or that upsets the laws of conservation of energy. True, we do not yet understand Inhibition; yet, no scientist thinks of explaining it, except within "*ordinary*" scientific laws. And any force which transforms any incoming sensory nerve-current, however detrimental, into flat quiescence without expenditure of other physical energy in opposition to it, certainly does not act within ordinary scientific laws.

force in evolution above all other forces. It acts upon *every* external stimulus, interrupts and transforms its natural effect, molds and remolds the entire organism and all subsequent species in accord with the non-physical and miraculous power. If such a factor be admitted so dominantly throughout nature then exact science becomes absolutely impossible.

Our examination of this "New Factor" may, therefore, now be summarized as follows. It is obvious, historically, how its ancient traditions, rooting originally in superstition, have survived and come down to be an anomaly in our scientific times. It never had any closer foundation in facts than the superficial observation that pleasure more often comes with health and strength, and pain with weakness and disease. The central neural processes on which it is alleged to be based do not exist. The phenomena in question, upon examination, flatly contradict at every point the assumptions and assertions boldly made of them. The alleged "Factor," if carried out under these assumptions and specifications so made of it, quickly reduces the entire realm of biology and of psychology to endless confusion and ridicule.

On the other hand these phenomena have now been treated of substantially without violating the symmetries of nature, and in accord with the obvious demands and analogies of the remainder of ascertained knowledge. Pain-nerves have been conclusively demonstrated. Pleasure and displeasure, if they have not been so successfully disposed of as bodily pain, have been finally divorced from it and from the tradition that they are "quality activities" of any kind; they are rapidly being driven by new analysis and definitions to where they are seen to be forms or movements of thought quite independent of specific qualitative make-up; are being explained on the same footing and in the same categories with conceptions, volitions and similar mental processes, which apparently may be of any and every "quality," or, at least, in which the qualities of the content play no at present determinable part.

Nor have these things been done in a corner. Modern literature is full of them. These new opinions have been put for-

wardly neither timidly, obscurely, nor by inferior men. Long ago so great an authority as Prof. James declared of the ancient tradition that it was "*one of the most artificial and scholastic untruths which remain to disfigure modern science.*" There would seem, therefore, now to be as little excuse for an intelligent man to believe in this "New Factor" as to continue to believe in the other half of the tradition, *i. e.*, in a personal devil. For a scientist to continue to throw such 'disfiguring untruth' among the already vastly complicated problems of biology and psychology, of heredity, and of social and ethical development, while completely and blindly ignoring the objections which have been heaped, mountain high, against it, cannot henceforth be counted as less than pure Orientalism. To persist in the attempt, with whatever sincerity and enthusiasm of purpose, can only result, as my first words portrayed, in retarding the swing of the pendulum to a more sober consideration by Science of the problems of mind, and in bringing our New Psychology to speedy and undeserved contempt.

It seems hardly worth while to follow Prof. Baldwin into the doctrines of "Imitation" and "Organic Selection" built by him upon his above foundation, when these foundations show themselves to be the veriest myths.

BIRDS OF NEW GUINEA (MISCELLANEOUS).

By G. S. Mead.

(*Continued from page 290.*)

The family of *Certheidæ* (Creepers) have but scant representation in New Guinea, the genus *Climacteris* furnishing the only specimens. One species is perhaps peculiar to the island, viz.: *Climacteris placens.* Its plumage above is dusky, tinged reddish on the head with black marks interspersed. Below grayish, spotted brown and black. Sexes alike. Length, six inches. Salvadori says the female has reddish cheeks.

One Nuthatch also belongs to New Guinea—*Sittella* or *Sitta papuensis*—a very small species, less than five inches in

length. Above, the feathers are brown barred with black; below, the arrangement and shading are similar; head and throat white, as are also the upper tail-coverts. Below they are dark, sometimes obscurely spotted. The tail is black and short. Bill black. Legs yellow. The female differs in the coloring of head and under parts, not always essentially.

Of the Megaluri scarcely more than two or three species are to be found within the boundaries of New Guinea. One of these is *Megalurus macrurus*, from the southeastern portion of the main-land. The bird is eight inches long; dull brown above, or at times brighter and tawny and streaked with black; under parts coarse white and bluff; and long tail, more than half the entire length. From the same region comes another species much smaller in every way, but of more varied coloring. Considerable white marks the little bird—the under parts, cheeks and quills of the wing feathers taking this hue. Above the ground color is a rusty brown, with black streaks and markings on the shoulders and head. Black prevails on the wing-coverts.

Cisticola exilis, with an endless string of synonyms, which it has obtained by its wide distribution and change of plumage, is a very small thrush, varying in length from 3.5 to 4 inches. In Australian form the head seems to be of a more even reddish or rusty hue. Otherwise the general color is plain gray, picked out with black along the neck and upper back. Sometimes the gray is tinged, as along the wings and tail. Around the face is much white and yellow. The under parts are a discolored gray or buff. The females differ in having the head touched with lines of black or deep brown, and generally in a deeper tone. The plumage changes with the seasons.

Among the Bubbling Thrushes the genus *Sericornis* is represented in New Guinea by two or three species. *S. beccarii*, from the Aru Islands, is colored above dusky, ferruginous on the rump and tail-coverts, and black edging on the wings. Some feathers show white points. Much brown appears in certain lights. White in streaks on the face. Throat white, slightly touched with black. The under parts are a discolored white flanked with brown.

S. arfakiana is very similar in general appearance and colorings. An obscure wing-bar may be traced on the brown wings. The head is darker than the back. The throat is ferruginous, the remaining under surface olivaceous. Length, 4.5 inches.

The beautiful family of the *Nectariniidæ* (sun birds), with their slender forms, their curving bills and metallic plumage, is well represented in New Guinea, rather numerically by individuals than by variety of species. *Cinnyris aspasiæ*, known also under an appalling number of synonyms, is black, green or blue, according to shading and locality, besides differing considerably in size. The green variety gives out a green gloss from the burnished surface of the back, while beneath the feathers are velvety black. Other reflections are to be observed in different lights. From the throat escape the loveliest blue tints. The larger form (*C. auriceps*), with its lovely golden-capped head, is a dark blue, and is found in several of the adjacent islands. *C. proserpina*, both small and large, is a black-shouldered form, throwing out green, blue and purple, according to the position of the beholder. All of these are of miniature size, and variants of the same general type.

C. frenata, the Australian yellow-breasted sun bird, with brilliant blue tints on its throat, is abundant in southern New Guinea and elsewhere, as well as at Cape York, where Moseley saw it. This species is yellow below, yellowish-green above. The female lacks the blue throat, but has bright gold instead over the entire under parts, from tail to the bill. It breeds in November and December, constructing a little purse of a nest with the covered entrance near the top. Within are laid the tiny eggs, colored dull green, and mottled with dusky spots. These repose on a soft bed of feathers and silky materials. In defense of his home, as indeed at almost all other times, the male is as belligerent as a humming-bird, attacking and putting to rout any vagrants loitering near. The total length of the bird is 4.5 inches, of which the bill comprises nearly an inch, and the short, narrow tail about the same. The latter member is black, with much white in spots on the outer feathers. Bill and feet black. The great beauty of the male lies in the metallic blue throat.

Of the group of Nectarinædine birds called *Arachnathera*, all confined to the Indo-Malayan region, three, if not more species belong to New Guinea exclusively. These are *A. polioptera*, *A. novæ guiniæ* and *A. iliolophus*, all about the same small size, and, owing to this fact, resembling each other to all appearance. The first named, *A. polioptera*, has a steel-blue gray head running into olivaceous, becoming yellowish green along the neck and back. On the wings and tail slate-blue takes the place of green, relieved along the edges by traces of gray or white. The under surface is yellow, retaining, however, the olivaceous tint of the upper parts. On the throat there is the usual changing hues, common to this class of birds. This species lives in the Astrolobe Mountains along with the *A. iliolophus*, although the latter seems more widely spread, being found as well in the southern portions of the great island. A special difference may be pointed out between the two birds, the general color is lighter, that is in the loosened, fluffy plumage of the lower back and sides. In this characteristic *iliolophus* has a marked advantage, the feathers becoming very soft and considerably elongated over the short tail. *Arathothera novæ guiniæ* is similarly adorned. Its breast, in fact, the under parts generally, is more brightly adorned than the foregoing, being of a brilliant yellow, dashed, however, with olive. In other respects the coloration is nearly the same—olive, olive-brown and brown predominating. There may be in the present case rather more yellow especially about the face.

The Javan Swallow—*Aviundo frontalis*—is only about five inches in total length, measuring from the tip of its tiny bill to either point of the deeply-forked tail. The general color above is dusky, scarcely the usual steely-blue of most swallows, but with a darker shade on the shoulder and crown. The under parts are much lighter, at least in the case of the New Guinea specimens, which are invariably pale; in fact, the abdomen is almost white. On the upper breast and throat a fine rufous tint is very prominent. The tail above is a uniform black; below there is a broad band of white following the triangular form of the fork, but melting at the apex into the wavy white of the coverts. The little bird is by no means con-

fined to Java, but is very widely distributed over the Papua, migrating to far distant tropic lands besides.

Hirundo nigricans or *Petrochelidon nigricans* presents few features of coloration to distinguish it from the various forms of swallows of different denominations so well known in all parts of the world. The present bird wanders widely over the Australian continent and Papua. It is small of size, not much over five inches in total length, although many specimens exceed this measurement, the individual differences being unusually great. Dun rumped, as Latham called this species, fairly characterizes it; but this appearance varies according to age, locality and season. In general color above, dark gleaming blue, faintly marked white lines prevails. Below, the body is buff or whitish, with a dark-hued breast. Wings above are dusky; below a ruddy tinge, tail feathers similar.

Many of the family *Dicæidæ* find a home in New Guinea or its islands. They are all small, usually prettily colored birds, allied at least in appearance to the sun birds, although hardly as elegant of form or richly plumaged as those delicate denizens of the tropics.

Dicœum rubrocoronatum abound in southern New Guinea, especially near Port Moresby. It is a showy little creature, a trifle over three inches in length only. Above the color is blue-violet, becoming less distinct on the neck, and merging into rusty on the wings. The tail continues this deep blue of the body; but over the rump and crown of the head a bright scarlet is thrown. This reappears in a broad segment on the upper breast. The female lacks this conspicuous ornament altogether, while the scarlet elsewhere becomes merely dull red. In other respects she is colored like her mate, having the under parts pale yellow, olive and white. She is, however, unmarked by a pinkish tinge on the under tail-coverts, which adorns the male bird. In both the middle abdomen and throat are a buffish white. Bill and feet dark.

Scarcely to be distinguished from the preceding is *D. pulchrius*, who differs chiefly in the coloration of the under tail-coverts, which, instead of pink, are a yellowish-brown. In lieu of this deficiency *pulchrius* has been granted a slightly

larger expanse of scarlet on the head and neck. The habitat is the southeast.

Another species from southeast New Guinea, along the Fly River, differs from the first named chiefly in having a glossy black upper surface instead of blue. It is known as *D. rubrigulare.*

Smaller and more plainly colored is *D. pectorale*, whose leading tints are olive-green above, yellow on head and rump, no scarlet whatever excepting on the upper breast, the remaining under parts and tail-coverts light yellow, exclusive of the whitish under wing-coverts.

From the Bay of Gielvink comes *D. gielvinkianum* or *mafoorense*, of an olive color above glossed with steel-blue. Here again a shade of red appears on the crown, rump, upper tail-coverts and breast. The under surface is a yellowish-white bordered along the sides with olivaceous. A more brightly tinted variety is named *D. jobiense.*

The genus *Oreocharis* of the Dicæidæ, represented by one species, is peculiar to New Guinea; this is *Oreocharis arfaki*, collected by Mr. Goldie in the Astralobe Mountains. This is a larger bird by two inches than those of an allied kind just considered. The color above is dissimilar, viz.: an olivaceous, somewhat glossed. The dusky wings, however, are touched with green and yellow on some of the feathers. So, too, the tail above. Crown of the head and sides glossy black, melting into bottle-green on the neck. About the eye are dashes of the brightest corn-yellow. This is the color also of all the under parts, excepting the black throat. The under wings are paler, with black touches. A reddish stripe may be seen in the gold ground of the under parts.

Urocharis longicauda is likewise the sole species of the genus *Urocharis*, and occupies the same region of the Arfak Mountains. Above the general color is a shining black, the only exceptions being the rump, which is gray, and the tail, where on the outer feathers a long spot is visible. The side face is olivaceous; this is the color on the under body mingled with pale yellow. The female is larger by more than half an inch, and is a smooth olive-green. Length, about five inches. The tail nearly half this figure.

The genus *Melanocharis* comprises four species, all from New Guinea and its islands. These are not very dissimilar in size or coloration. The best known, well named *M. nigra*, is a glossy black above of a bluish cast. Beneath the principal tint is olivaceous, passing into pale yellow on the abdomen. The under wing-coverts are white. Total length, nearly five inches.

Another genus of the same family, consisting of but a single species, is *Pristorhamphus veroteri*. A larger bird this by an inch, with rich, velvety black plumage above, emanating pale green. Underneath a bluish tint. Besides, some spots of white on the tail, apparent when the bird is flying, but concealed at other times; there are white plumes, very soft and delicate, waving on either flank. The female is equipped with these same adornments, but is of dimmer coloring, mainly olivaceous. Habitat, the Arfak Mountains.

Less by more than an inch is *Rhamphocharis crassirostris*, the sole member of its genus. An olive-green bird above with dusky brown wing- and tail-coverts, and blackish tail. Below the body is a pearl-gray with a yellow wash. The female is of larger size, olive-brown above, but differing from the male in being rather more varied in neutral colors, yellow and white spots or dots appearing on the dull surface of wings, tail and back. The under parts are of a soiled white, specked with yellow and brown. The bill is not noticeably larger than that of other species.

THE BACTERIAL DISEASES OF PLANTS: A CRITICAL REVIEW OF THE PRESENT STATE OF OUR KNOWLEDGE.

By Erwin F. Smith.

II.

I. THE BEET (BETA VULGARIS L.).

1. THE BACTERIOSIS OF FODDER BEETS (1891).

(I) The disease.

(1) *Author, Title of Paper, Place of Publication, etc.*—This disease was first described by Dr. Ernst Kramer, Privat Docent

in the technical high school in Graz. His paper entitled (**23**) Die Bacteriosis der Runkelrübe (*Beta vulgaris* L.), eine neue Krankheit derselben, was published in *Oesterreichisches Landwirtschaftliches Centralblatt*, Jahrg. I, Heft 2, pp. 30 to 36, and Heft 3, pp. 40 to 41. Graz, 1891.

(2) *Geographical Distribution.*—The disease prevailed extensively in 1890 in the Eltzischen earldom in Vukovar, Slavonia.

(3) *Symptoms.*—The beet roots were said to be shrivelled and to contain comparatively little sap. The whole of the affected roots began to change to dark brown soon after harvest. On cutting them open dark brown spots were visible. During the winter the disease spread in the beet cellars to apparently sound beets, in spite of the fact that all roots showing any signs of disease were thrown out and destroyed at the time of storage. Roots in which the disease was well advanced showed a gummy ooze which appeared to be infectious to sound beets. Cattle fed with slightly infected roots were attacked with severe bloating and obstinate constipation, and in one case death ensued. Such was the account forwarded to Dr. Kramer along with samples of the diseased beets. There is no record of the symptoms of this disease as it occurs in the field. The diseased beets received by Dr. Kramer were shrunken and in some places were soft under the epidermis. From these soft places there oozed a slimy brownish fluid, which stuck to the fingers, but was without characteristic smell or taste. Brown or dark brown spots more or less softened, and of various sizes, were visible on cross-sections of roots not too badly infected. The inside of those specimens which were badly attacked was, however, almost entirely brown, and in parts the parenchyma was wholly destroyed, giving place to a slimy, sticky, gum-like, brown-colored, strongly acid, odorless fluid. The destruction of the tissues proceeded so far in some parts of the root that, finally, only the vascular bundles remained. The beets attacked by this disease yielded no characteristic odor, and they only began to smell bad in the last stages of the disease after rotting had set in.

(4) *Pathological Histology.*—An examination of thin sections, made through a brown spot, showed that the cells of this

parenchyma contained tiny roundish or ellipsoidal shining bodies, which were of various sizes, and either scattered about in the cells or united into groups. The individual particles showed not rarely a tremulous motion. When such a thin section was fixed to a cover glass by passing it three times through the flame, and was then stained with gentian violet, these bodies became a beautiful blue, and their bacterium-like form could be made out more clearly. In the parts of the root which had already become slimy great numbers of bacteria were to be seen in the gum-like fluid, together with loosened cells, plasma, and fragments of cell membranes.

(5) *Direct Infection Experiments.*—When a little of the soft, slimy mass was lifted on a sterile platinum needle and spread on a sterile [steamed?] section taken from an apparently sound beet, the surface of the latter was covered within forty-eight hours with a slimy, brown, gum-like, acid layer, which consisted of a mass of those bacteria previously found in the diseased beets. Sections cut out of diseased beets with sterile knives and placed on fresh, unsterilized sections from sound beets, and kept in a moist chamber at 24° C., caused the latter to become affected. The infected spots browned and softened, and in the tissues bacteria appeared, which were just like those occurring in the diseased beets. A slimy layer also formed on the sterile cut surface of carrots when a slight quantity of the slimy ooze from the beets was spread over it.

"This preliminary investigation indicated that most likely in this case we have to do with a disease caused by bacteria. Positive proof, however, is not thereby afforded. To accomplish this experimentally it is absolutely necessary to isolate the bacteria occurring in the diseased beets, to cultivate them pure, and then to inoculate the pure cultures into sound living beets. If then as a result of the infection the previously healthy beet should become diseased with the before-mentioned symptoms, and the originally inoculated bacteria should appear once more in the tissues, then there would be no doubt about this being a bacteriosis of the beet."

Clearly this man knew exactly what he had to do.

(II) THE ORGANISM.—This is described as a bacillus, but not named.

(1.) *Pathogenesis.*

(A) Yes.

(B) Yes. Rather easy.

(C) No. These inoculations led to no satisfactory result, and had to be abandoned, because no suitable beet material was at the experimenter's disposal.

(D) No.

Conclusion.—Pathogenic nature rendered probable. The reason for this judgment in opposition to the above statements will be found in the following paragraph.

While Dr. Kramer was not able to secure infections, owing probably to the unfavorable conditions under which he worked, he hit upon an ingenious method of indirect proof, viz., the development in pure cultures of the same gum which is formed naturally in the diseased beets. His method was as follows: The softened or liquefied parts of the diseased beets were cut out, crushed and heated on a water bath, with the addition of a small quantity of milk of lime. The fluid was then decanted, and the remaining mass of beet squeezed as dry as possible and the two fluids mixed, filtered, and carbon dioxid passed into the filtrate for the removal of the somewhat superfluous lime. The fluid was again filtered and concentrated on the water bath. The fluid was now rendered acid by the addition of some drops of acetic acid, and a white, tough, gum-like substance was precipitated out of it by the addition of 96 per cent. alcohol. To obtain it in a pure condition this substance was repeatedly dissolved in water and reprecipitated by alcohol. The same substance was obtained directly from the gummy ooze of the diseased beets by dissolving it in water, heating, filtering, concentrating on the water bath, and precipitating with alcohol. In this case also the precipitate was a white, tough, gum-like substance. Both of these precipitates were tested chemically with the following results. Mixed with soda-lime and heated in a test tube there was no formation of ammonia, a proof that the substance was free from nitrogen. Boiled with orcin and hydrochloric acid it gave the well-known gum reaction, men-

tioned by Reichl and Wiesner. Boiled with sulfuric acid it was converted into dextrose. A watery solution gave a bluish flocculent precipitate with Fehling's solution, and on boiling the latter was reduced. On the addition to a watery solution of ferric chlorid and calcium carbonate the well-known precipitate of gum solutions resulted. No red coloration appeared on treatment with iodin. The formation of oxalic acid could not be detected on long boiling with nitric acid. All these reactions indicated a gum. In the beet this could be derived only from carbohydrates, and most likely from dextrose. Working on this hypothesis, a fluid culture medium was prepared containing 3–4 per cent. of dextrose, a slight quantity of peptone, and the necessary mineral ingredients. In this solution pure cultures of the organism were grown 8 to 14 days at a temperature of 24° C., and from the resulting products of growth a gum-like substance was obtained which proved to be identical with that secured directly from the diseased beets. These cultures were protected from contamination by cotton plugs, and at the close of the experiment cultures therefrom showed them to have remained pure, consequently this bacillus must have converted the dextrose into gum.

2. *Morphology.*

(1) *Shape, size, etc.*—The organism as isolated and grown in pure cultures is a thick rod with rounded ends, or often narrowed at the ends (zugespitzt), of variable length, so that not rarely coccus or ellipsoidal forms appear. These rods are about 1.30–2.00 x 0.7–1.0 μ. In cultures they occur singly or in pairs, which latter are more or less biscuit-shaped. Chains are rarer.

(2) *Capsule.*—No mention of any capsule.

(3) *Flagella.*—No statement as to motility, except mention of the trembling motion inside the cells of the beet, which can scarcely be taken as a proof of motility.

(4) *Spores.*—"Apparently spores are formed." This matter is left in considerable doubt. Rods in the stage of spore formation are said to be 1.35 x 2.00 μ.

(5) *Zoogloea.*—No mention of zoogloea.

(6) *Involution forms.*—No mention of any distorted forms.

3. *Biology.*

(1) *Stains.*—These bacilli take all the ordinary anilin colors.

(2) *Gelatin.*—On plate cultures of nutrient gelatin containing dextrose the colonies are small, nearly circular, sharply contoured, white, shining, and at most not over 1 mm. in diameter. Under a weak magnification they appear sharp-edged, granular and brownish. Stab cultures in the same gelatin show a fine thread not spreading beyond the needle track. At the mouth of the stab there is a top-shaped enlargement not inclined to spread out much. Streak cultures on nutrient gelatin develop a line along the track of the needle which is very slightly inclined to widen. This is formed of dot-shaped hyaline colonies, which finally fuse. The bacillus does not liquefy gelatin.

(3) *Agar.*—Plate, stab and streak cultures on nutrient agar were not unlike those on the gelatin. Exact statements as to the composition of the nutrient gelatin and agar are not given.

(4) *Potato, etc.*—Pure cultures on sterilized slices of beet gave a brownish, slimy growth, having a strongly acid reaction. The same on carrot gave a whitish, slimy layer, having a strongly acid reaction. On potato the growth showed no specially characteristic mark, but was strongly acid.

(5) *Animal Fluids.*—No statement.

(6) *Vegetable Juices.*—No statement.

(7) *Salt Solutions and other Synthetic Media.*—The 3–4 per cent. dextrose-peptone solution (distilled water?) containing the necessary mineral ingredients (not named) became cloudy in forty-eight hours and less limpid in 8 to 14 days.

(8) *Relation to Free Oxygen.*—Aerobic.

(9) *Reducing and Oxidizing Power.*—No statement.

(10) *Fermentation Products, and other Results of Growth:*

(a) *Gas Production.*—No statement. If the cattle disease were really due to this organism, then we may suppose it to be an active gas producer in the presence of certain carbohydrates.

(b) *Formation of Acids.*—This bacillus is a strong acid producer. The sap of the diseased beets shows a strong acid reaction. Pure cultures strongly reddened blue litmus gelatin in

forty-eight hours, and developed an acid reaction in neutral nutrient solutions within a few days. The composition of these solutions is not stated, nor whether the growth of the organism is self-limited by the production of the acid. This production of acid serves to strengthen the belief that the bacillus is really the cause of the beet disease. The nature of the acid was not determined.

(c) *Production of Alkali.*—Not recorded.

(d) *Formation of Pigment.*—Brownish color on beets.

(e) *Development of Odors.*—No odor.

(f) *Enzymes.*—Not determined. Cell walls are dissolved.

(g) *Other Products.*—Not stated.

(11) *Effect of Dessication.*—No statement.

(12) *Thermal Relations:*

(a) *Maximum for Growth.*—Not determined. If the bloating of the cattle were due to this organism, it must be able to grow at blood heat, and the accurate determination of its thermal relations should not have been omitted.

(b) *Optimum for Growth.*—Not determined.

(c) *Minimum for Growth.*—Not determined.

(d) *Death Point.*—Not determined.

(13) *Relation to Light.*—No statement.

(14) *Vitality on Various Media.*—No statement.

(15) *Effect on Growth of Reaction of Medium* (*acid, neutral, alkaline*).—No statement.

(16) *Sensitiveness to Antiseptics and Germicides.*—No statement.

(17) *Other Host Plants.*—No statement.

(18) *Effect upon Animals.*—No cattle could be had for experimental purposes; but the germ was carefully tested on rabbits and white mice, and was not pathogenic either when fed to them in carrots, rubbed into subcutaneous wounds, or injected into the blood by means of a Pravaz syringe.

(III) Economic aspects:

(1) *Losses.*—Serious.

(2) *Natural Methods of Infection.*—Not known.

(3) *Conditions Favoring the Spread of the Disease.*—Not known.

(4) *Methods of Prevention.*—No experiments, and no observations. Disease not studied in the field.

Remark.—This disease was also seen in 1891 by Dr. Paul Sorauer, who described it as follows, in a short note appended to a (**24**) "Review" of some papers on the Sereh-disease of sugar cane (*Zeitschrift für Pflanzenkrankheiten*, Bd. I., Heft 6, 1891, p. 360). "We can now report similar phenomena in our *Beta*. A parcel of beets sent to us from Slavonia were suffering from a disease which may be designated *gummosis*. Investigations up to this time have shown that the bacteria induce the formation of a syrup-like gum. Here also the first indications of the disease are a red-brown, subsequently a black-brown, staining of the vascular bundles, and each drop of gum swarms with myriads of apparently specific bacteria. If this gum is dropped upon sliced (*praeparierte*) sound beets the bacterial gummosis is there easily produced. The preparation of the beet so as to be susceptible to the disease appears to lie in a lessening of the acidity of the tissues, etc." No strictly bacteriological work appears to have been done, and I have quoted all of the article that is pertinent.

2. THE ROT OF SUGAR BEETS (1891).

In 1891, in (**25**) *Fungous Diseases of the Sugar Beet*, Bull. No. 15, Iowa Agric. Experiment Station, Ames, Iowa, p. 243, Reprint p. 9, Prof. L. H. Pammel, of that station, described a beet disease from Iowa which he attributed to the fungus *Rhizoctonia betæ*.

Associated with this fungus were various bacteria to which he ascribed the subsequent wet rotting of the roots. The rotting beets gave off a strong odor not unlike that of rotting potatoes. Unquestionably "the ultimate rotting is caused by bacteria." Several bacteria were isolated, and among others *Bacillus subtilis*. Inoculations with a pure culture of one of these organism did not give any very decisive results. No bacteriological studies of any consequence seem to have been made.

3. A BACTERIAL DISEASE OF SUGAR BEETS (1892).

(I) THE DISEASE.

(1) *Author, Title of Paper, Place of Publication.*—This disease was described by Dr. J. C. Arthur and Katherine E. Golden, of

the Purdue University Agricl. Experiment Station, Lafayette, Indiana, in (**26**) *Diseases of the Sugar Beet*, issued April 13, 1892, and forming Bulletin No. 39, Vol. 3, of that station, pp. 54–58, and summary, pp. 61–62.

(2) *Geographical Distribution.*—This disease appeared in sugar beets grown for experimental purposes at the Indiana station, and seems to have first attracted the attention of the station chemist owing to the low percentage of sugar found in some of the roots. These were examined microscopically, and bacteria, or bacteria-like bodies found in the tissues. This was in 1890. "Owing to the lateness of the season, and the lack of a plant house, the observations on the disease soon came to an end, to await the next growing season. The following description of the disease, and of its distribution and cause, is therefore the result of studies made almost wholly during the summer of 1891 and the winter of 1891–2." The disease is prevalent in many places in Indiana. In 1892 it occurred in all of the eight varieties of sugar beets grown on the Purdue Station grounds, and was found to some extent in nineteen of the twenty-seven samples of beets sent in for analysis from as many different localities in that State. This is not, however, it is stated, an entirely fair indication of the prevalence of the malady, since it is customary to select the best beets for analysis, and the proportion of diseased ones in such lots is less than the actual average. Of a total of 434 beets received from different parts of Indiana, and examined for this disease, 12 per cent. were affected. No record was kept of the percentage of diseased beets appearing on the experiment station grounds, but this is stated to have been large.

(3) *Symptoms.*—"This disease does not usually cause the death of the plants, any spots upon its surface, or any alteration or discoloration of the tissues."

"The beet root shows externally no marks by which the presence of the bacterial parasite can be detected; the most diseased and the strictly healthy roots cannot be separated by any external characters. This statement, however, does not apply to the leaves. While the plants are small, the foliage of healthy and diseased plants remain normal; but as the plants

reach full size, and especially as they approach maturity, those which are most affected can be told at a glance by the altered appearance of the leaves. The healthy beet leaf has a decidedly flat, uniform surface, while the diseased leaf is puffed out between the veins in little blister-like areas, giving the general appearance of the surface of a Savoy cabbage leaf. Diseased plants are necessarily less vigorous than healthy ones, and the fact is made apparent to the eye as the season advances, by the leaves becoming paler and smaller, and the outer ones dying away faster than upon healthy plants. All these indications taken together, most reliance being placed upon the crinkled surface, will enable one to select much diseased plants as they are growing in the field, with considerable certainty. But some roots not showing the foliage characteristics will also be found to be affected.

"Upon cutting across a root the most constant indication of the malady is a greater prominence of the fibres which form the concentric rings. In well-marked cases each microscopic bundle shows a dark dot, the circles of dots growing more distinct on exposure to the air. In less pronounced cases the woody fibres are merely yellowish, or even quite colorless, but more prominent after being exposed to the air for awhile than normal tissues. Furthermore, the diseased root is rather soft and tough, and of a yellowish-white color, while a healthy root is firm, somewhat brittle, and in color a clean white. It has also been found that diseased roots are lighter in weight than healthy ones." They also contain less sugar. The reduction in all cases being considerable, and in some cases amounting to nearly 50 per cent. This is "presumably due to the presence of the bacteria."

In a foot-note it is stated that the circles of dark dots are found in all sugar beets. "And yet the greater prominence which these dark spots assume on account of the disease, make them one of the most effective indications of its presence."

(4) *Pathological Histology.* — A microscopic examination showed the bacteria "throughout all parts of the root." "So far as observed the disease rarely or never breaks down the tissues or kills the plant."

"A section from any part of a diseased root, under a magnification of four or five hundred diameters, shows the presence of great numbers of bacteria." "These bacterial parasites of the beet are not few, or difficult of detection; but occur in great numbers in every cell of the plant, and are conspicuous under the microscope without staining or other special treatment. The more pronounced the disease the greater the number of bacteria. They are most abundant in the large, loose-celled tissue, lying between the fibrous rings of the root and in the similar tissue of the veins and midrib of the leaf. This tissue consists of parenchyma, in which the protoplasm lines the walls of the cells and stretches across in strings from side to side. The bacteria are largely imbedded in or attached to the protoplasm, but also occur in the cell sap, sometimes in large numbers. While the bacteria are most abundant and conspicuous in the colorless parenchyma, they also occur in the cells of the fibro-vascular bundles, and in the green cells of the leaf; in fact, as has already been said, in all parts of the plant."

(5) *Direct Infection Experiments.*—No direct inoculation, or grafting of diseased roots upon healthy ones, appears to have been tried.

(II) THE PARASITE. Organism not named.

1. *Pathogenesis.*

(A) Yes.

(B) Yes. Easily isolated. Plate method. From the deeper tissues of the roots only one form of microbe is obtained.

(C) No, or yes, doubtfully. "Inoculation with pure cultures into the beet root has been attempted, and results appear to show that the disease was transmitted; but the trials were few, and we desire to repeat them before further discussing this part of the subject."

(D) No.

Conclusion.—Pathogenic nature not established.

2. *Morphology.*

(1) *Shape, size, etc.*—The bacteria are all of one shape and appearance. They are nearly twice as long as broad, small,

oblong, colorless, usually occurring as colorless cells, although occasionally found in pairs. No measurements are given.

(2) *Capsule.*—Nothing.

(3) *Flagella.*—The organism is said to be actively motile when grown in a rich nutrient fluid. No statement regarding flagella; or as to motility when taken directly from the plant.

(4) *Spores.*—Said to be arthrosporous; but no details are given or proof advanced in support of this statement, which probably rests on no other foundation than that endospores were not observed. So far as known to the writer nobody has demonstrated the existence of arthrospores in any species of bacterium or bacillus.

(5) *Zooglœa.*—No mention of zooglœa.

(6) *Involution forms.*—No mention of involution forms.

3. *Biology.*

(1) *Stains.*—No statement respecting behavior toward stains.

(2) *Gelatin.*—"Upon neutral gelatine the bacteria at first form a whitish growth, which becomes pale yellow with age, and the gelatine is eventually liquefied. Upon acid gelatine the liquefaction proceeds much more slowly. In all cases the gelatine finally becomes alkaline, whether acid or neutral to begin with."

(3) *Agar.*—"Upon agar-agar the growth is about the same as upon acid gelatine."

No statement as to what nutrient substances were added to the agar or to the gelatin or as to the reaction of the agar.

(4) *Potato, etc.*—Behavior not stated.

(5) *Animal Fluids.*—Not stated.

(6) *Vegetable Juices.*—"Develop well in sterilized juice expressed from the sugar beet; but their development cannot be readily watched, as contact with the air causes the juice to turn dark or even black."

It is not stated whether the expressed juice was sterilized by steam heat or at ordinary temperatures by filtration.

(7) *Salt Solutions and other Synthetic Media.*—"In a Pasteur sugar culture the bacteria grow well, causing the liquid to become slightly turbid in twenty-four hours. As growth

goes on, the turbidity becomes greater, and again decreases until at the end of nine or ten days, when growth practically ceases, the liquid becomes clear, and a grayish sediment falls to the bottom of the tube."

No statement as to whether the sugar in this solution was broken up with the formation of an acid.

(8) *Relation to Free Oxygen.*—No statement. Certainly not anaerobic, from the ease with which cultures were obtained.

(9) *Reducing and Oxidizing Power.*—No statement.

(10) *Fermentation Products and other Results of Growth*:

(a) *Gas Production.*—No statement.

(b) *Formation of Acids.*—No statement.

(c) *Production of Alkali.*—Neutral or acid gelatin becomes alkaline. In view of this statement it would be interesting to know whether the juice from diseased roots is alkaline, or less acid than that from healthy roots.

(d) *Formation of Pigment.*—Old cultures on gelatin are pale yellow.

(e) *Development of Odors.*—No statement.

(f) *Enzymes.*—Gelatin is finally liquefied.

(g) *Other Products.*—No mention of any.

(11) *Effect of Dessication.*—No statement.

(12) *Thermal Relations:*

(a) *Maximum for Growth.*—Not determined.

(b) *Optimum for Growth.*—Not determined.

(c) *Minimum for Growth.*—Not determined.

(d) *Death Point.*—Not determined.

(13) *Relation to Light.*—No statement.

(14) *Vitality on Various Media.*—Not recorded.

(15) *Effect on Growth of Reaction of Medium* (*acid, neutral, alkaline*). Liquefaction of gelatin delayed by acidity.

(16) *Sensitiveness to Antiseptics and Germicides.*—No statement.

(17) *Other Host Plants.*—None recorded.

(18) *Effect Upon Animals.*—No statement.

(III) ECONOMIC ASPECTS:

(1) *Losses.*—"A source of danger to the beet sugar industry of no inconsiderable moment." See also (I) (2).

(2) *Natural Methods of Infection.*—Not determined.

(3) *Conditions Favoring the Spread of the Disease.*—Not determined.

(4) *Methods of Prevention.*—No experiments recorded, and nothing known.

Remark.—From the statement quoted under *Pathogenesis* (*C*) one might infer this to be a preliminary paper, and it is possible that a subsequent one may clear up some of the many mooted points. At present the most that seems to be made out beyond doubt is that there is in Indiana a disease of sugar beets accompanied by decreased sugar content, and always or usually associated with minute bodies distributed pretty uniformly through the parenchymatic tissues, and believed to be bacteria. To the writer of this article the evidence that bacteria are really the cause of this disease does not appear to be very conclusive. Until more proof is advanced it is permissible to doubt (1) whether the organism isolated by plate cultures, and supposed to have been derived from the interior of the beets, was actually so derived; and (2) whether the bacteria-like bodies which "occur in great numbers in every cell of the plant," but which "never break down the tissues," or cause "any alteration or discoloration of the tissues," are really micro-organisms. To have every cell full of aerobic bacteria, and no lesions, is very remarkable, considering the nature of the plant cell, and certainly requires unusually strong evidence. Under the circumstances is it not possible that these bodies may be of a crystalline or crystalloid nature? This seems the more likely, from the fact that the juice of healthy table beets, the only sort the writer has been able to examine, is full of small particles endowed with active Brownian movement, and readily mistaken for bacteria when examined in hanging drops with medium magnifications. The uninjured parenchyma cells of the petioles were also found to contain these bodies in large numbers and in active motion. They stain slowly with alkaline methyl blue, and are not microörganisms.

4. THE DEEP SCAB OF BEETS (1891).

In (**27**) *Bulletin No. 4*, Agric. Experiment Station, North Dakota, Fargo, N. D., Dec., 1891 (pp. 15–17), Prof. H. L. Bolley,

of that station, described "a disease of beets indentical with deep scab of potatoes," which latter he attributes in another article in the same bulletin to "a bacterioid fungus-like affair having characteristics which would seem to ally it both to the fungi and to the bacteriaceæ." Inasmuch as this beet disease has frequently been cited on Prof. Bolley's authority as of bacterial origin, *e. g.*, in the last edition of Frank's (28) *Krankheiten der Pflanzen*, Bd. 2, p. 27, it is proper to mention it here, although the evidence for and against the bacterial nature of scab will be taken up seriatim only when we come to consider the bacterial diseases of the potato, to which the reader is referred.

5. THE ROOT-BURN OF BEETS (1894).

This disease should perhaps also be included. What little we know about its bacterial nature is derived from the brief account by Dr. L. Hiltner, in an address entitled, (29) Mittheilungen aus d. K. pflanzenphysiologischen Versuchsstation Tharand: Wie kann der Landwirt durch richtige Wahl, Pflege und Bestellung des Saatgutes des Krankheiten der Kulturpflanzen einigermassen verbeugen? *Sachsische Landw. Zeitschrift* 1894, No. 18, pp. 207–209.

Dr. Hiltner states that a disease called "Wurzelbrand" has caused great injury in recent years in almost all beet-growing lands. This disease appears in an early stage of growth as a more or less extensive constriction at the junction of stem and root. Subsequently there is a browning and decay of the root which proceeds from the constricted portion, and usually ends in the death of the plant. In spite of numerous investigations the cause of this disease has not been satisfactorily determined. For a long time its symptoms were confused with the gnawings of a beetle, *Atomaria linearis*, but Hellriegel, and afterwards Wimmer, showed that the disease could be preventod by soaking the beet balls for twenty hours in one-half per cent. solution of carbolic acid, and on this ground ascribes the disease to a fungus, which was believed to pass over from the beet balls to the roots of the young seedlings. Hollrung, on the contrary, found a fungus in the diseased parts of only four out of sixteen roots examined for that purpose, and ascribed the disease to

other causes, *i. e.*, to physical, chemical and mechanical peculiarities of the soil (Hiltner's account). Dr. Hiltner's own observations date from the discovery of a browning of the root hairs. He states that often in his germination experiments he had observed that a part of the root hairs on certain seedling beets would be colored brown and peculiarly shortened. When examined under a hand lens these hairs were seen to be mere brown points instead of long tubes. Seeds from a lot which germinated well, and produced seedlings that showed this browning of the root hairs to a marked degree, came up badly when planted in garden earth, and those which did grow afterwards developed the root-burn, the characteristic constriction occurring just where the sound root hairs were wanting. Hellriegel's treatment was repeated. After soaking the beet balls in a solution of carbolic acid the root hairs remained perfectly sound, and there was no subsequent root-burn. The parasite, however, is not a fungus but a bacterium. "In each epidermal cell of the root which bore a stunted hair there was to be found a specific bacterium, and to this is to be attributed the final destruction of the root." It is not stated whether the organism was isolated from the roots, or whether any infection experiments were undertaken. The context would lead one to think nothing of this sort was attempted.

RECENT LITERATURE.

Journey Through Mongolia and Thibet.[1]—This volume is 413 pages, is published in octavo form under the auspices of the Smithsonian Institution. It is an account of the travels of Mr. Rockhill in Mongolia and Thibet, based on a diary kept during the journey. The variety of subjects touched upon by the author in his descriptions of the country traversed, and the people with whom he was brought in contact, gives this volume a peculiar interest. Appendices to the diary

[1] Diary of a journey through Mongolia and Thibet in 1891 and 1892. By William Woodville Rockhill. Published by Smithsonian Institution, 1894.

contain information regarding the Solar and San-Ch'uan T'ujen vocabularies, the plants of Thibet, and the mean monthly temperature. A route map of the explorations made by the author, and a table of latitudes, altitudes, etc., accompany the book. The illustrations are numerous, comprising page plates and cuts in the text.

Publications of the United States Geological Survey for 1893–4. FOURTEENTH ANNUAL REPORT.[2]—This work is issued in two parts. Part I containing the Report of the Director on the operations of the Survey of 1892–92 administration, together with several reports. Part II comprises the accompanying papers embodying the results of the topographic and hydrographic work of the survey, and of the geographical investigations carried on through its aid. These researches were prosecuted chiefly in Virginia, West Virginia and Maryland, in Vermont and Massachusetts, in certain Rocky Mountain areas, and the Pacific coast region.

The illustrations are numerous, comprising 76 plates and 75 figures and diagrams.

A MANUAL OF TOPOGRAPHIC METHODS.[3]—This manual is one of the series of monographs published by the United States Geological Survey in quarto form. Its object, as stated by the author in his introduction, is to present a description of the topographic work, instruments and methods used by the United States Geological Survey primarily for the information of the men engaged upon this work. The manual is accompanied by a collection of constants and tables used in the reduction of astronomical observations for position, of triangulation, of light measurements, and other operations connected with the making of topographic maps, and is illustrated by eighteen plates showing types of topography.

MINERAL RESOURCES OF THE UNITED STATES.[4]—This report for the calendar year 1893 is the tenth in the series. The subject-matter includes: First, a statement of the mineral products; secondly, the industrial conditions affecting these products; and thirdly, recent additions

[2] Fourteenth Annual Report of the United States Geological Survey, 1892–93. Part I.—Report of the Director. Washington, 1893. Part II.—Accompanying Papers. Washington, 1894.

[3] Monographs of the United States Geological Survey, Vol. XXII. A Manual of Topographic Methods. By Henry Gannett. Washington, 1893.

[4] Mineral Resources of the United States for the Calendar Year 1893. By David T. Day. Washington, 1894.

to the knowledge of mineral deposits in this country. The statistics of production, and of imports and exports, were collected by experts, whose names are given at the heads of the several chapters, and are, therefore, unquestionable. The arrangement of the material is convenient for reference, and a good index completes the work.

An Introduction to the Study of Zoology.[5]—This book, as stated by the author, is a kind of guide-book to beginners in the study of the animal kingdom. Among other good points made by Mr. Lindsay is a recommendation of a course of study in his advice to students, and suggestions as to the best books to buy for those whose time or money is limited. This forms Part III. Parts I and II treat respectively of the general principles of the subject and systematic zoology. Part I is concise, but clear, and on the whole up to date. The systematic part, however, is weak, by reason of the lack of clear, precise definitions. This is particularly true of the Vertebrata; and, in general, no advantage has been taken of the discoveries of paleontology.

The illustrations are numerous and "taking." On the whole, the volume will be of interest and value to those whose wants it is intended to meet, *i. e.*, the adult student, who wishes a first-lesson book which is not milk for babes.

The Cranial Nerves of Batrachia.[6]—This paper is a reprint in book form of an article published in the *Journal of Morphology*, Vol. X, No. 1. The author confines himself to a discussion of the V, VII IX and X nerves, including other nerves in the description only as they come into connection with those specified. After an explanation of the technique employed, a detailed description of the nerves and their components is given, followed by a comparative morphology of components. The closing chapter deals with the relation of the cranial and spinal nerves, the relations of the pre- and post-auditory nerves, and the bearings which the results of the author's studies have upon the classification of the nerves and their segmental relations.

Especial light is thrown on this subject by this research, which includes as an especial feature the determination for the first time of the motor and sensory fibres in each case. The monograph is one of especial excellence.

[5] An Introduction to the Study of Zoology. By B. Lindsay. London, 1895, Swan, Sonnenschein & Co. New York, Macmillan & Co. $1.60.

[6] The Cranial Nerves of Amphibia. By Oliver S. Strong. Boston, 1895. Ginn & Co., Pub.

Structure and Life of Birds.[7]—In this volume the author undertakes to show the development of birds from reptilian ancestors from anatomical evidence. He then describes the structure in detail, dwelling on the work done by special organs. The chapter on flight comprising some hundred pages, contains the latest information on this vexed problem, and includes much original matter, the result of Mr. Headley's personal investigations and observations. Color and song, instinct and reason, migration, and the principles of classification are treated of in separate chapters. A brief recapitulation of the arguments for and against the theory that the ancestors of the ostrich family were birds of flight, and hints as to the best methods of studying ornithology, are given in concluding the subject. Each chapter is accompanied by a list of books bearing upon the topic under discussion.

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[7] Structure and Life of Birds. By F. W. Headley. New York and London, 1895. Macmillan & Co. $2.00.

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General Notes.

MINERALOGY AND CRYSTALLOGRAPHY.[1]

The Chemical Composition of Turquoises.—Carnot[2] notes the occurrence of turquoise in the Burrow Mts., Grant Co., N. M., in a sort of pinkish-gray pegmatite. The structure is micro-crystalline, the fracture irregular and somewhat conchoidal. The analysis given is: P_2O_5 28.29, Al_2O_3 34.32, CuO 7.41, FeO .91, MnO trace, CaO 7.93, MgO trace, H_2O 18.24, F trace, quartz or clay 2.73, total 99.83. An analysis of the well known Persian turquoise gave P_2O_5 29.43, Al_2O_3 42.17, CuO 5.10, FeO 4.50, H_2O 18.59, quartz or clay .21, total, 100.00.

These analyses and others already published show, it is true, a good deal of variation in the composition of turquoise, yet are thought by Carnot to agree fairly with the formula P_2O_5 (Al_2 Cu_3 Fe_3 Ca_3) O_3+ Al_2O_3+5 H_2O. Stress is laid on the determination of *all* the iron as ferrous. The above data were obtained from the true oriental turquoise, or that "of the old rock."

The occidental turquoise, or that "of the new rock" may better be called odontolite, coming from the teeth of fossil mammals. They are very variable in composition, and contain iron in the ferric condition, as well as 3.02 per cent, or, in another specimen, 3.45 per cent of fluorine, thus differing from the oriental turquoise.

The occidental turquoise may be distinguished from ordinary bones and fossils by lack of calcium carbonate, presence of ferric phosphate, and by the large quantity of aluminium phosphate, also by the blue color.

Alstonite and Barytocalcite.—A posthumous note by Mallard[3] presented to the French Society of Mineralogy by M. Termier, gives interesting comparisons between the properties of the minerals containing barium and calcium carbonates. While barytocalcite has been long considered to be a double salt, the usual view concerning alstonite has been that it is an isomorphous mixture of the two carbonates. A series of analyses made by Chatelier suggests that alstonite may be also a double salt with the same formula as barytocalcite. The prismatic

[1] Edited by A. C. Gill, Cornell University, Ithaca, N. Y.
[2] Bull. Soc. Fr. Min., XVIII, pp. 119-123, 1895.
[3] Bull. Soc. Fr. Min., XVIII, pp. 7–12, 1895.

angle of alstonite is determined as 119° 9′, which is not in accord with the view that it is an isomorphous mixture of witherite and aragonite, since their corresponding angles are 117° 48′ and 116° 16′ respectively.

The indices of refraction of witherite, alstonite and barytocalcite for sodium light were measured and compared with those af aragonite and calcite. In the following table, column III gives the mean between the values for aragonite and for witherite:

	I Aragonite	II Witherite	III Mean	IV Alstonite	V Barytocalcite	VI Calcite
α	1.5301	1.529	1.5295	1.525	1.525	1.48625
β	1.6816	1.676	1.679	1.673 (?)	1.684	
γ	1.6859	1.677	1.681		1.686	1.6585
Sp. G.	2.94	4.28	3.61	3.71	3.65	2.73

Attention is called to the remarkable crystallographic similarity between barytocalcite and calcite, nothwithstanding the difference in crystal system. The cleavage of barytocalcite form a pseudorhombohedron, being basal and prismatic. The angle of the prism 106° 54′, and the angle between the base and prism is 102° 54′, while the cleavage rhombohedron of calcite has angle of 105° 5′. Moreover, the optical angle of barytocalcite is small, and the negative acute bisectrix make an angle of +64° 22′ with the c axis (i. e., with the intersection of the prismatic cleavages); the optical angle of calcite is zero, and the negative optical axis makes an angle of +63° 44′ with the intersection of two rhombohedral cleavages.

In conclusion Buchrucker's values for the indices of strontianite are corrected. Mallard's values for Na light are: $\alpha = 1.518$, $\beta = 1.664$, $\gamma = 1.665$.

Rutile, Cassiterite and Zircon.—According to Traube,[4] who discusses the question of the isomorphism of the above minerals, the etched figures produced by KF or KF HF are exactly similar for each of the three species, and indicate holohedral symmetry in the tetragonal system.

An attempt to make rutile containing SiO_2 was not successful, though Traube considers that it must have been so in case rutile and zircon were isomorphous. Synthetic experiments were also made for the purpose of throwing light on the mode of occurrence of Fe_2O_3 in these minerals. By heating chemically pure amorphous titanium dioxide in a platinum crucible with sodium tungstate and a metallic oxide, the following results were obtained. With Fe_2O_3 rutile was formed containing in one case as much as 5.4 per cent of that oxide; rutile

[4] Neues Jahrb. B.B. X, pp. 470–476, 1896.

with 3.01 per cent Mn_2O_3, and in another case with 1.91 per cent Cr_2O_3 was prepared, but similar experiments with the oxides of nickel and cobalt were not successful, the rutile crystals containing no appreciable trace of Ni or Co. Chromiferous crystals of cassiterite were also formed. It seems, therefore, that these substances have a peculiar affinity for the oxides of the type R_2O_3, but not for those of the form RO.

Colored specimens of all three minerals become permanently lighter in color on heating.

Marignac's process for fusing zircon (i.e., with KF or with KF HF) was tried with rutile and cassiterite. Like zircon they both fuse rather readily, forming $K_2\ TiF_6$ and $K_2\ SnF_6$ respectively.

Miscellaneous Notes.—Wülfing[5] describes a simple apparatus for obtaining monochromatic light from direct sunlight. The experiments on quartz seem to show that the apparatus works with a good degree of accuracy. Measurements of the index of refraction of diamond gave for A, n=2.4024; for D, n=2.4175; and for H, n=2.4652. These are three of several values determined. The specific gravity of these diamonds referred to water at 4° was found to be 2.522±.003. Hematite from Elba was also investigated, giving:

ω	ϵ	
2.904	2.690	for line A
2.988	2.755	for line B
3.042	2.797	for line C

The specific gravity at 4° is 5.285±.002. A description is also given of a spectrum apparatus for use with a microscope or an axial angle instrument. In a later note[6] Wülfing gives a table comparing the values of the indices of refraction of the diamond obtained by himself with those determined by Walter; the agreement is very close. He states that either apparatus above mentioned may be obtained of Eug. Albrecht in Tübingen.

Kretschmer[7] describes the occurrence of garnet, vesuvianite, wollastonite, epidote, augite, quartz and calcite at the contact of marble with granite near Friedeberg in Silesia. Minute details as to locality, association and crystal form are recorded.

Goguel[8] reports on the crystal form, and in some cases on the optical behavior of formopyrine, $C_{12}\ H_{14}\ N_2O_2$, and its addition salts with

[5] Tscherm. Mitth., XV, pp. 47–76, 1895.
[6] Ibid, p. 350.
[7] Tscherm. Mitth., XV, pp. 9–28, 1895.
[8] Bull. Soc. Fr. Min., XVIII, pp. 27–31, 1895.

hydrochloric, sulphuric, nitric, phosphoric and oxalic acids.—Duparc and Pearce[9] have measured the crystal angles and observed the optical properties of eight new chemical compounds. These are benzoyl-malic acid, sodium orthophenyl-benzoate, potassium orthophenyl-benzoate, ammonium phenyl-glycolate, dextrocinchonine phenyl-glycolate, benzylic ether of bromo-tolu-quinone oxime, potassium luteo-phosphomolybdate and a potassium luteo-phosphotungstate.

Of late numerous additions have been made to our knowledge of the crystallographic and optical constants of organic compounds. The following three papers in Volume XXV in the Zeitschrift für Krystallographie may be cited as important contributions to this line. 1. The Crystal form of Some New Halogen Derivatives of Camphor, by F. S. Kipping and W. J. Pope; 2. On the Crystal Form of Some Organic Compounds, by W. J. Pope; 3. Crystallographic and Optical Investigations on Some Organic Compounds, by E. A. Wülfing.

An artificial cassiterite investigated by Arzruni[10] shows distinct dichroism with the ray vibrating parallel to the vertical axis colorless, while the ray vibrating at right angles thereto is pink. The crystals reach a half centimeter in thickness and twice that in length. Twins, which are so common with natural cassiterite, were not observed. The angles measured coincide within 2′ with those given by Becke for the natural mineral. The mean values from two determinations of the indices of refraction are:

	Li	Na	Tl
ω	1.9846	1.9968	2.0093
ϵ	2.0817	2.0929	2.1053

These numbers agree as well as could be expected with those obtained by Grubenmann for cassiterite, showing that the natural and artificial products are practically identical.

Schmidt[11] gives at great length tables showing the recurrence of like interfacial angles in the regular system. As an extreme example, the angle 35° 15′ 52″ occurs between eleven pairs of faces, the cube, octahedron or dodecahedron constituting one face of each pair. The table at the end of the paper may be of use for rapidly identifying rare faces on regular crystals.

Sohncke[12] shows that in accordance with his views of crystal structure no circular polarization is to be expected in crystals of the pyramidal

[9] Bull. Soc. Fr. Min., XVIII, pp. 31--43, 1895.
[10] Zeitschr. f. Kryst., XXV, pp. 467--470, 1895.
[11] Zeitschr. f. Kryst., XXV, pp. 477--503, 1895.
[12] Zeitschr. f. Kryst., XXV, p. 529, 1895.

tetragonal class (hemimorphic hemihedral division of the tetragonal system). So far as known, circular polarization is not exhibited by crystals with this grade of symmetry.

PETROGRAPHY.[1]

Petrography of the Bearpaw Mountains, Montana.—The Bearpaw Mountains are the dissected remains of a group of Tertiary volcanoes. Their cores of the old volcanoes are granular rocks, their lavas and tuffs are represented by basic sheets and beds. The lavas are largely basalts, leucite-basalt and other similar basic types.[2]

The cores consist of mica-trachytes, quartz-syenite, porphyries, containing aegerite-augite and anothoclase-phenocrysts, in which are imbedded microlites of oligoclase, trachytes containing hornblende and diopside and shonkinite. A few miles from Bearpaw Peak a denuded core is exposed, which furnishes a good example of the differentiation of a syenite in place. The intrusion is laccolitic in character. Around its borders it has highly altered the sedimentary rocks with which it is in contact. The most acid portion of the laccolite is a light aplitic syenite containing quartz and diopside. The main mass is a more basic syenite resembling monzonite or yogoite. It contains diopside and much plagioclase. The most basic phase is a shonkinite. Analyses for the three principal types follow:

	SiO_2	Al_2O_3	Fe_2O_3	FeO	MgO	CaO	Na_2O	K_2O	H_2O_5	Other	Total
Quartz-syenite	68.34	15.32	1.90	.84	.54	.92	5.45	5.62	.45	.57 =	99.95
Monzonite	52.81	15.66	3.06	4.76	4.99	7.57	3.60	4.84	1.09	1.86 =	100.24
Shonkinite	50.00	9.87	3.46	5.01	11.92	8.31	2.41	5.02	1.33	2.68 =	100.01

The totals corrected for Fe and Ce are 99.94, 100.22 and 99.93 respectively.

Two French Rocks.—In the serpentine of St. Préjet-Armadon, Haute-Loire, France, Lacrou[3] finds nodules composed of asbestiform gedrite surrounding a kernel of serpentine or biotite. The nodules are separated from the serpentine by an envelope of biotite. They are sup-

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.
[2] Weed and Pirson: Amer. Journ. Sci., IV, Vol. 1, p. 283 and 351.
[3] Bull. Soc. Franc. d. Min., XIX, p. 687.

posed to be of secondary origin. Bronzite and asbestus both occur in the rock. In the norite area of Arvien, Auvergne. the same author describes a variety of this rock which is characterized by the presence of secondary reaction, rims of anthophyllite and actinolite between its hypersthene and plagioclase, the former appearing next to the pyroxene. The plagioclase of the rock is often altered to actinolite, garnet and albite, while the hypersthene is changed to an aggregate of anthophyllite.

The Granite of the Himalayas.—McMahon[4] describes the granite of the N. A. Himalayas. Although highly foliated in the borders of its masses, the rock is shown to be eruptive. The author thinks the foliation is due to pressure upon the rock before it finally solidified. He attempted to show that this schistosity could not possibly have been produced after the rock cooled. The granite is coarsely porphyritic with large orthoclase crystals in a medium to fine grained groundmass composed of the usual constituents of granite. This is cut by tiny veins of quartz which are supposed to represent the micrystallized residue left after the first partial consolidation of the rock, or to be the result of a partial fusion of the quartz grains originally occurring in it. This quartz, though it presents the usual aspects of secondary quartz, is thought to have been injected into the vein spaces while it was in a molten condition. Sinuous areas and viens of microcrystalline mica are likewise observed in the granite, and these are thought to have been produced by the rapid crystallization of mica that had been melted, and not by the crushing and shearing of the original micas nor by secondary processes of any other kind. The paper is well illustrated by photo-micrographs.

California Rocks.—Fairbanks[5] describes the rocks of Eastern California between Mono Lake and the Mojave desert as comprising both sedimentary and igneous forms. Among the latter are both granitic and volcanic varieties. The granites form the eastern slope of the Sierra Nevadas. In the northern portion of the area it is a coarsely porphyritic biotite hornblende variety. In the southern portion it is replaced by a more basic phase containing less hornblende. The volcanic rocks met with in the district are andesitic flows, dykes and tuffs, and basalt flows among the more recent rocks and liparites among the more ancient ones. The microscopical description of the type is deferred to a later paper.

[4] Proc. Geologists Association, Vol. XIV, p. 287.

[5] Amer. Geologist, Vol XVII, p. 63.

Turner[6] gives a classification of the igneous rocks studied by himself from various places in California. He divides them into families in accordance with their mineralogical composition, including in the same family all those rocks with the same composition irrespective of structure. He then takes up the syenites and discusses them in some detail. The family is made to include syenites (granular), syenite-porphyries (porphyritic) and trachytes (microlitic and glassy) and apo-trachytes. The syenites include soda-syenite or albitite, augite-syenite, hornblende-syenite and mica-syenite. The apo-trachytes include among other rocks Rosenbusch's orthophyres and keratophyres. Until very recently no rocks of the syenite family have been proven to occur within the borders of the State. All those rocks described as such are now known to be hornblende-andesites, granites or diorites. The author refers briefly to the known occurrence of the syenites in the State and describes more fully some new ones.

He reports dykes of white albitite-porphyries or soda-syenite porphyries in the rocks of the Mother lode quartz mines. In the bed of Moccasin Creek the rock consists of quartz, muscovite and albite, but in other places it consists almost exclusively of albite with a few grains of an olivine-green mineral thought to be aegerite. The rock resembles somewhat Brögger's sölosbergite and Palache's albite rock containing crossite. An analysis of one specimen gave:

SiO_2	TiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	K_2O	Na_2O	H_2O	P_2O_5	Total
67.53	.07	18.57	1.13	.08	.55	.24	.10	11.50	.46	.11	=100.34

Gabbro-Gneiss from Russell.—The gabbro of Russell, St. Lawrence Co., N. Y., is said by Smyth[7] to change its character rapidly in consequence of a variation in grain from moderately fine to very coarse, in structure from porphyritic to granular and in color from black to gray. Upon alteration the gabbro passes into a rock made up of red masses in a groundmass of gabbro. In other places it becomes schistose, when it takes on a granulitic texture. Sometimes hornblende is developed in it in long narrow plates that run approximately at right angles to the schistosity, causing the rock to resemble a metamorphosed sediment. Even in the most gabbroitic varieties of the rock the plagioclase is changed into an aggregate of secondary products, among which scapolite is the most common. In the change of the massive gabbro into the schistose variety the constituents are first

[6] Ib., Vol. XVII, p. 375.
[7] Amer. Jour. Sci., Vol. 1, p. 273.

granulitized and then drawn out into lenticular areas. The feldspars of the gneisses appear to have been recrystallized, since the feldspathic areas consist of single feldspar individuals and not fragments of grains. The pyroxene also differs from the gabbro pyroxene. It has lost its characteristic black inclusions and has assumed a deep green color. This mineral, as well as the hornblende, which is abundant in the gneisses, are both regarded as having recrystallized, the augite material coming from the original augite of the gabbro and the hornblende from the secondary amphibole so common in the gabbro. The gneisses are thus schistose gabbros in which recrystallization has taken place with attendant granulitization. The author points out the fact that in the first stages in the alteration of the gabbro scaly hornblende and scapolite are formed, while in the final stage they have completely disappeared, and in this latter stage there results a gneiss which bears no evidence of having been crushed.

Notes.—The serpentine near Bryn Mawr, Penna., has resulted by the alteration of a peridotite according to Miss Bascom.[8] The rock of the Conshohocken dyke is a typical diabase.

GEOLOGY AND PALEONTOLOGY.

Fossil Jelly Fishes.—Certain curious forms, locally known as "star cobbles," have long been found in the middle Cambrian shales and limestones of the Coosa Valley, Alabama. They occur at two horizons associated with silicious concretions. The "star cobbles" are recognized by Mr. Walcott as fossil medusæ, and among the 8,000 specimens now in the collections of the U. S. Geological Survey he has separated two types allied to the recent Discomedusæ. From the large number of specimens that have been found over a relatively small area, it is evident that they were gregarious and very much like the modern Rhizostome (*Polyclonia frondosa*) in their habits.

The author describes three species, and refers them to two new genera, Brooksella and Laotira, which he also defines. These forms, *Brooksella alternata*, *B. confusa* and *Laotira cambria*, together with *Dactyloidites asteroides*, the author groups in the family Brooksellidæ, and gives a diagnosis of the family.

[8] Proc. Amer. Acad. Science, 1890, p. 220.

A number of drawings of different views of the three specimens described accompany the paper. (Proceeds. U. S. Natl. Mus., Vol. XVIII, No. 1086, 1896.)

Is Palæospondylus a Mansipobranch?—In a collection of fossil fishes belonging to Columbia College, Mr. Bashford Dean has found a specimen of Palæospondylus presenting structural details which decidedly oppose the hitherto accepted view that Palæospondylus is a paleozoic lamprey. Mr. Dean figures the specimen in question, and presents the positive and negative evidence as to its marsipobranchian affinity. From this summarized statement it is seen that the cranium, vertebral column and paired fins do not bear out the theory; the caudal fin is essentially marsipobranchian, but its diphycercal, or, perhaps, heterocercal condition, is also common to many groups, shark, lung fish, and teleostome. The only characteristic which Palæospondylus retains allying it with the Cyclostomes is the presence of tentacles in the anterior head region. The author suggests that in the presence of so much negative evidence the head-tentacles can hardly be taken as a crucial test of kinship, since it is quite possible for these structures to have arisen independently within the group to which Palæospondylus belongs. (Trans. N. Y. Acad. Sci., Vol. XV.)

The Skeleton of Aepyornis.—A small collection of remains of the extinct birds of the genus Aepyornis, obtained from central Madagascar, has been sent to England by Dr. Forsyth-Major. The specimens include portions of two skulls, two imperfect mandibles, some coraco-scapulæ, a nearly perfect sternum, and some small bones supposed to be rudimentary humeri. A detailed description of these bones is given in a recent number of *The Ibis* by Mr. Charles W. Andrews. Of the skull he remarks that "in several respects Aepyornis approaches the Dinornithidæ in the structure of the skull. Among the points of resemblance are the pedunculate occipital condyle, the prominent basi-temporal platform, the open Eustachian groove, the structure of the facet for the quadrate, and the presence of the frontal crest of large feathers (as in some of the Dinornithidæ)."

The sternum is "ratite," and in Apteryx is found the closest resemblance to Aepyornis. According to the author the fossil sternum appears to lack a metasternal region, and consists of the two primitive costosternal elements only. In this respect it corresponds to an embryonic stage in the development of the sternum in the recent Ratitæ.

The coracoscapula is typically Struthious in form. It similarity to that of Casuarius gives support to Milne-Edwards and Grandidier's

opinion that Casuarius is a near ally of Aepyornis. (The Ibis, July, 1896.)

Geological News. MESOZOIC.—Mr. C. W. Andrews has published a paper on the structure of the Plesiosaurian skull, in which he institutes a comparison of the palatal portion with that of other Reptilia. He shows that while a similarity of structure in that region does not necessarily imply close relationship, nevertheless the very great resemblances existing between the Plesiosaurian and Rhynchocephalian palates, reinforced by the numerous other points of likeness in other portions of their skeletons, lead to the conclusion that the Sauropterygia, notwithstanding their single temporal arcade and the rhizodont dentition, are descended from a primitive Rhynchocephalian reptile. This conclusion is in accord with the opinion already expressed by several writers. (Quart. Journ. Geol. Soc., May, 1896.)

CENOZOIC.—A restoration of *Hoplophoneus occidentalis* Leidy has recently been completed by Mr. E. S. Riggs, under the direction of Dr. Williston. The material upon which its restoration is based is composed of parts of two skeletons found almost together and in exactly the same horizon just below the *bullatus* layer of the Oreodon beds of South Dakota. This material now forms part of the paleontological collection of the University of Kansas. (Thesis for the Degree of A. M. in the Kansas Univ., 1896.)

—In a paper on recent and fossil Tapirs, Mr. J. B. Hatcher describes a new species of Protapirus from the Protoceras beds of the White River (Oligocene) of S. Dakota, presenting some new facts as to the osteology of the skull and forelimb of this genus. He also gives additional characters diagnostic of the various species of Protapirus and Colodon already described by Leidy, Marsh, Wortman, Earle and Osborn; points out the distinctive osteological and dental characters in the skulls of the five generally accepted species of recent Tapirs; and reviews the previous work of others on the Phylogeny of the Tapiridæ and Helaletidæ. (Amer. Journ. Sci., Vol. I, 1896.)

—A restoration of the skeleton of *Aptornis defossor* has been completed for the British Museum (Nat. Hist.). The bones from which the specimen is reconstructed were found in 1889 in a chasm in the limestone at Castle Rocks, Southland, New Zealand, the greater number of them no doubt belonging to a single bird. Mr. C. W. Andrews gives a brief description of this skeleton, calling atten-

tion to the great size of the anterior vertebræ in the cervical region, and the peculiar long, slender coracoids which are ankylosed with the much reduced sternum. The figure accompanying the text shows the probable position of the scapula in relation to the coracoid, the coracoscapular angle being very obtuse, as in most flightless birds; the humerus is proportionally small, and its pectoral crest is reduced to a mere tubercle. (Geol. Mag., London, June, 1896.)

GENERAL.—According to C. D. Perrine, thirty-three distinct earthquakes were felt in California during the year 1894. This does not include a series of over one hundred shocks in Virginia, Nev., during the week of November 16–22, nor heavy earthquakes and volcanic disturbances which occurred in the New Hebrides group of islands during October and November. (Bull. U. S. Geol. Surv., No. 129, Washington, 1895.)

BOTANY.[1]

The Teaching of Elementary Botany.—That the teaching of elementary botany in this country is, to say the least, very poor, is a statement which needs no argument to prove its correctness. Much of the botany of the public schools is a wretched attempt at doing something which neither teachers nor pupils understand. In some places the pupil is made to con the pages of a text-book in which emphasis is laid upon minute and meaningless anatomical details of the structure of a few flowering plants. Elsewhere field-work, so-called, is required of the pupil; but here again the chaff is carefully separated from the grain, and *the pupil is given the chaff.* Thus he is made to fill out vacancies in blanks (called "schedules") in which the unimportant structural characters receive as much attention as those which are significant, the result being a description which neither describes nor separates the plant under consideration from dozens of others. The meaning of any structure is entirely overlooked, while the pupil is compelled to give much time and labor to unimportant details.

There are two reasons for this condition of things: first, the little knowledge of the science of Botany possessed by many teachers; and second, the absence of any definite idea on the part of teachers of the culture-value of Botany in the education of the pupil. To remedy the first the colleges and universities are opening summer schools for

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

teachers; and in these, for the most part, something of modern Botany is given. Many years of personal experience has shown the writer that in one Western State it has been possible to do much in the way of improving the teaching of Botany through the agency of the summer school. Let not the teachers of Botany in the colleges and universities grudge the time given to work in the summer school. The additional work is doubtless the most productive work of the year, for, if it be well done, its effects will be felt by hundreds of pupils in many schools. Let professors put their best efforts and their most mature thought into this work.

The remedy for the second obstacle may be looked for in the movement in the National Educational Association, which resulted in the organization of a Department of Natural Science Instruction, whose first meeting was held recently in Buffalo. It was notable that every paper presented at this meeting emphasized the culture-value of Science, and this was especially marked in those dealing with Botany in the school curriculum. We, who teach in the larger colleges and universities, have been remiss in not setting forth more prominently and forcibly the culture-value of Science, and botanists have sinned equally with the others. It is high time that we not only teach Botany for the culture which it gives the student, but we should by lectures, public addresses, and by popular articles, show how it may be presented so as to insure culture. Here we have a duty to perform, and if we have the interest of Science truly at heart, we will not shrink from the labor which this duty imposes. Let every professor of Botany realize that through the new department of the National Educational Association he may influence the teaching of his science so that it may have a culture-value.—CHARLES E. BESSEY.

The Conifers of the Pike's Peak Region.—It may help the visitor to Colorado Springs and Manitou to know that the following conifers are more or less common in the adjacent mountains. Perhaps, when he learns that through the carelessness of man enormous forests of these trees have been burned from the sides of Cheyenne Mountain, Cameron's Cone and Pike's Peak, and that where once grew dense forests of conifers, with their power of conserving the moisture of the snows and rains, there grows the worthless "popple" (*Populus balsamifera candicans*), he, too, will be ashamed of man, the vandal, who has destroyed forever, I fear, the conifer forests of this region, with the destruction of which forests there has been a decrease in the volume of water in the mountain streams, while at the same time the sudden and dangerous floods which rush down the mountain sides have greatly increased.

Juniperus communis alpina, the Mountain Juniper, is common everywhere from 7000 feet altitude to timber-line (11,500), as a low, spreading and almost trailing shrub.

Juniperus occidentalis monosperma, the Brown Cedar, or more commonly called here by the erroneous name of White Cedar, is common in the Garden of the Gods.

Juniperus virginiana, the Red Cedar, is to be found in the Garden of the Gods and generally at low altitudes. Some of the trees are entirely clothed with the short, blunt leaves, giving them a smoothness not generally seen in this species. Such trees are more glaucous, and are more round-topped than the ordinary kind in which many of the leaves are sharp-pointed.

Abies concolor, the White Fir, occurs abundantly from about 7000 feet to 8000 or 10,000 feet above sea level. Its beautiful layered foliage and erect cylindrical cones make it an object of interest to every traveller.

Pseudotsuga taxifolia (*P. douglasii* of Coulter's Manual), the Douglas Fir, is the most common of the single-leafed conifers, occurring everywhere from the foot of the mountains to timber-line. It is distinguished at once by its elliptical cone, with trifid bracts between the scales.

Picea engelmanni, Engelmann's Spruce, and *P. pungens*, the Sharp-leaved Spruce, are common from 7000 or 7500 feet up to 9000 or 10,000 feet altitude on the eastern slopes of Pike's Peak.

Pinus flexilis, the Rocky Mountain White Pine, occurs from Cheyenne Mountain to Pike's Peak, from 7000 feet to timber-line, where it is very common. It may readily be distinguished by its leaves, *which are in fives*.

Pinus balfouriana aristata. This tree resembles the preceding, and apparently is often confused with it under the name of "White Pine" or "Foxtail Pine." It grows at high altitudes (10,000 feet) up to timber-line, and in this region is a small, or at most, a moderate-sized tree. Its short leaves (about one inch) which are in fives, and prickly cones distinguish it from all other species.

Pinus edulis, the Nut Pine, is a low, spreading tree, often not more than ten or twelve feet in height. It may be distinguished by its short leaves and small cones, the latter containing a few large edible seeds. It is common in the Garden of the Gods and on the foot-hills a few hundred feet higher.

Pinus ponderosa scopulorum, the Rocky Mountain Yellow Pine, or more commonly called the Bull Pine, is the most abundant conifer of the region. It grows at all elevations, from the foot of the mountains and foot-hills to timber-line. Its leaves are long, and occur in twos and less commonly in threes.—CHARLES E. BESSEY.

Ferns Near Colorado Springs, Colorado.—So many thousands of travellers visit the beautiful city of Colorado Springs every year that the following list of the ferns to be found within easy walking distance from the end of the car lines may be of interest to botanical readers.

Notholæna fendleri Kunze.
Pteris aquilina L.
Cheilanthes tomentosa Link.
C. fendleri Hook.
C. gracilis (Fee.) Mett.
Pellæa atropurpurea (L.) Link.
Asplenium trichomanes L.
A. filix-foemina (L.) Bernh.
A. septentrionale (L.) Hoffm.
Phegopteris dryopteris (L.) Fee.
Dryopteris filix-mas (L.) Schott.
Cystopteris fragilis (L.) Bernh.
C. fragilis dentata Hook.
C. bulbifera (L.) Bernh.
Woodsia scopulina D. C. Eaton.
W. oregona D. C. Eaton.
W. obtusa (Spreng) Tore.
Botrychium virginianum (L.) Swz.
B. matricariaefolium A. Br.

A few notes on the above list may be of interest. There is a good deal of individuality about the Colorado climate, and the same is true of its ferns and their habits. The *Woodsia* and the *Pteris* are almost the only ferns found on the open hillsides, and these but sparingly; the others seek the protection of the mountain cañons. Most of them prefer cañons opening toward the north. During three summers spent in Colorado I do not remember finding a single fern in any canon opening toward the south.

In *Notholæna fendleri* we are told that the pinnules are oval in mature specimens. In most young fronds I have found them deltoid or spatulate, and in some beautiful specimens this form is retained. In such ferns the stipes are lighter in color and weight, the zigzag course of the rachis is less pronounced, the pinnæ are more distant, and the pinnules less numerous, giving the specimens a much lighter and more graceful appearance. The departure from the normal form is worth noting, but not sufficient to constitute a variety.

Cheilanthes tomentosa, according to the books, is from eight to fifteen inches in length at maturity. Most specimens live up to the rule, but

not a few ignore the books, and mature their fruit before they are eight inches tall; indeed, some very saucy specimens refuse to grow beyond a single inch, and scatter their spores to the winds in spite of their insignificant size.

The two ferns named above are to be found in most of the shady cañons near Colorado Springs, but *Asplenium trichomanes* I have found only in one place in South Cheyenne canon, while *A. septentrionale* has not been seen outside of the beautiful gulch in the Ute Pass, from which the city of Manitou obtains its water supply. All the lower canons of the Ute Pass would be rich fields for the botanist if the vandal tourist could be kept out of them; as it is, there are still a few treasures left on the high rocks and in out of the way corners. Here *Phegopteris dryopteris* flourishes and *Cystopteris* runs riot.

The two Botrychiums were found iu North Cheyenne Cañon, *B. virginianum* four, and *A. matricariaefolium* eight miles from the mouth of the cañon. Naturally, such fleshy ferns are seldom found in the dry atmosphere of Colorado, yet, in the one station where found, *B. matricariaefolium* was quite plentiful, and varied in form from a simplex-like plant of two inches to beautiful specimes nine inches high.

Of *Cystopteris fragilis* Eaton wisely wrote "very variable." The same might well be written of the whole genus so far as Colorado is concerned. It is the most abundant fern on Cheyenne Mountain, and there flourishes with little regard for the specific fences within which the books expect it to grow. I have not included *C. montana* (Lam.) Bernh. in the above list, because specimens found are hardly as broad as the typical form that species demands, while too broad to be classed as *C. fragilis*. The *C. bulbifera* found is without bulbs, but otherwise conforms to the books. The "winged" or "wingless rachis" of the books is not an unfailing test in differentiating the Colorado species of this genus, a microscopical examination of the indusium being necessary.

But if the Colorado *Cystopteris* is "very variable" what shall I say of Woodsia? I have entered *W. scopulina*, *W. obtusa* and *W. oregona* on the above list, because from the large amount of material on hand it is easy to select specimens which exactly conform to the species type in the books. I believe also that some specimens answering to *W. mexicana* might be selected, while a few would almost pass for *W. alpina*. But when this has been done what are we to call the still larger number of specimens, which are not exactly *W. scopulina*, nor *W. oregona*, nor *W. mexicana*, nor *W. obtusa?* Shall we say they are Woodsia, simply Woodsia, and nothing more? It seems to me that in this genus there is work waiting to be done of the same wise sort that Mr. George E. Davenport did some years ago in the genus *Botrychium*.—Alford A. Butler.

ZOOLOGY.

Lygosoma (Liolepisma) Laterale in New Jersey—As this species (the Oligosoma laterale of Girard and authors) is mainly characteristic of the South Atlantic and Gulf States—the Austroriparian Region in brief—its occurrence in New Jersey under such circumstances as to lead one to believe it a regular member of the fauna is of interest as an additional fact showing the strong southern stamp which the fauna and flora of that interesting region bear.

Yarrow's check-list, and most published lists since, record Salem, N. C., as the most northerly locality in the eastern United States, while southern Illinois and Indiana mark the northernmost limit of distribution in the Mississippi valley. Prof. Cope, however, informs me of a record for Maryland.

The New Jersey record is based upon a single specimen taken near Batsto, in Burlington County, on May 29th, of the present year. It was found on the ground concealed beneath a wood pile on a deserted farm, and glided away so quietly, clinging so closely to the ground and so skillfully seeking concealment beneath every small plant and chip, that it almost escaped unperceived.

It remained an interesting captive for about one month, but finally succumbed to its appetite in attempting to dispose of a large Polydesmus entire. During its captivity it partook of several small specimens of Julus, Polydesmus, and pill-bugs (Armadillo), besides small beetles and flies, a larval grasshopper and an earth-worm (Allurus)—food very different from that selected by *Sceloporus undulatus* under similar circumstances.

It was very fond of water, and sipped it up eagerly when poured on the bottom of its jar; the snout was buried beneath the surface, and the slender tongue travelled out and in rapidly until the water had sunk beneath the surface of the sand.

A similar movement of the tongue took place, as in the snakes, when food was detected, or under other excitement. The body movements were exceedingly graceful, but slow, as compared with Eumeces or Sceloporus.

During the night of June 14th two eggs were laid, and were found the next morning on the surface of the sand, without any covering whatever. Sceloporus has a similar habit in captivity, my experience being that the eggs are only partially or not at all covered. These eggs of

Lygosoma have the usual tough parchment-like shell, with calcareous surface deposits, as in many Reptilia. They have a structure essentially similar to that described for the egg-shells of Pityophis. They are more slender in form than those of Sceloporus, and measure 9.5 x 4.5 mm. The contained embryos were somewhat younger than those of Sceloporus usually are at the time of disposition—that is, the allantoic outgrowth was but just visible. The fertility of this specimen renders it probable that the species is a regular habitue of the neighborhood, but its small size, and retiring and nocturnal habits, render it very likely to escape notice.—J. PERCY MOORE.

On a New Glauconia from New Mexico.—Nasal entirely divided, rostral rounded behind, reaching the line of the eyes. Two labials anterior to the ocular, the posterior reaching the eye. Frontal and supraorbital scales smaller than those posterior to them. The eye is close to the nasal, and distant from the supraocular. Postocular reaching last labial, and bounded posteriorly by three sublingual scales. Inferior labials five, the second twice as large as any of the others; the fourth barely reaching the commissure of the mouth, and the fifth very small. Scales in fourteen rows. A large preanal plate. Tail flattened below, entering total length about fifteen times.

Color very light-brown above, whitish below. Total length 235 mm.; tail 12 mm.

I found the specimen above described in a road at the silver mines at Lake Valley, southern New Mexico.

The appearance of this species is so similar to that of the *G. dulcis* that I originally identified it with the latter. It is, however, very different, especially in the number of labials, and the scales which adjoin the postocular posteriorly. There is no plate comparable to the so-called parietal of *G. dulcis*. I propose that it be called *G. dissecta*.—E. D. COPE.

On the Habits of Keen's Deer Mouse, Peromyscus keeni (Rhoads).—The following interesting notes were forwarded me by the Rev. J. H. Keen, a missionary on the Queen Charlotte Islands. The Deer Mouse referred to was originally described in the Proceedings of the Academy of Natural Sciences for 1894, from specimens procured by Mr. Keen, and forwarded to Philadelphia. The remarks on the use of the cheek pouches for the conveyance of food are of particular value. It has been known for many years that several species of this genus possessed cheek pouches; but I can remember no personal observation of their use by the living animal, having been published.—SAMUEL N. RHOADS.

Acad. Nat. Sci., Phila., July 16, 1896.

MASSETT, QUEEN CHARLOTTE ISLANDS,
BRITISH COLUMBIA, February 22, 1896.

Samuel N. Rhoads, Esq., Philadelphia, Dear Sir:—The following notes I have made lately on the character of *Sitomys keeni* may be of interest. Use them as you think best.

Sitomys keenii is the common house mouse here, and specimens are very numerous. I recently confined an adult female in a fairly spacious cage with glass front, I subsequently introduced three other nearly adult specimens. At first the old female resented the intrusion, but soon became reconciled. The younger ones may have been her offspring, having been taken in the same place. On two other occasions I introduced an adult male taken in another locality, whereupon the old female in each case attacked the intruder fiercely, chased him all over the cage till he was exhausted, and then flew at his thoat and bit him so severely that he died almost immediately. A shrew introduced later she treated in the same manner.

After a couple of days they became reconciled to confinement, and indifferent to being watched whilst feeding or at play. They ate bread moistened with milk and raw potatoes, but showed a marked preference for wheat. The wheat they never ate on the spot, but filled their pouches with it, and ascending a sloping board deposited it in their sleeping place. This they did with great rapidity, and a handful soon disappeared. The average number of corns taken at one mouthful was ten, but once or twice the old mouse took as many as sixteen. The first few corns they took up with their mouths, but used their feet to cram in the rest. When their pouches were full their heads were twice their normal size, and their expression extremely droll.

The storing propensity is evidently very strong. It is quite a common occurrence to find any empty article, which has been unused for a few days, half full of rice or corn when next taken up. Boots and shoes seem to be the favorite storing places; but a neighbor of mine, on visiting his outhouse, found the oven of a disused stove half full of rice, which had been obtained from sacks close by.

When in a trap, frightened, these mice sound an alarm by suddenly contracting the nails of the fore-foot so as to cause a sharp scratch on the floor. This they repeat several times, using sometimes one fore-foot, sometimes the other, but never the hind-feet.

Sitomys keenii is the only mouse here; *Mus musculus*, though on the opposite mainland, not having yet found its way thus far. In the summer of 1894 *S. keenii* was unusually numerous. An Indian crossing to

the mainland observed one in his canoe when in mid-ocean, and on reaching mainland saw it jump ashore and escape.

Yours faithfully, J. H. KEEN.

The Inheritance of an Acquired Character.—*Editor American Naturalist:* It has been my fortune recently to have brought to my notice an instance illustrating Darwin's theory of the origin of species, that seems to me noteworthy.

A certain Mr. J. B. Perry, a resident of Cleveland, is the owner of a very fine female fox-terrier, which has recently given birth to a litter of seven puppies, five of whom are remarkable from the fact that they were *born with short tails.*

These five were male puppies, while the two with tails of ordinary length are female.

Of the five short-tailed dogs one has almost no tail at all, it being but a little stump not over half an inch long.

When I examined them they were just two weeks old and barely had their eyes open. The tails of the females had been recently cut, and the scar on the stump was plainly perceptible, while the ends of the five short tails were entirely grown over with hair, and plainly were born in the condition I found them. Their length was about half the ordinary length, or about what is considered the "proper thing" by dog-fanciers, except in the case of the one already mentioned as having almost no tail at all.

When it is remembered that the custom of cutting off over half of the caudal appendage of the fox-terrior has prevailed for many generations back in the ancestry of a thoroughbred, the birth of short-tailed dogs is not to be wondered at. Yet this instance is so striking that it seems worthy of being brought to the notice of the readers of the AMERICAN NATURALIST.—NORMAN E. HILLS.

The Hartebeest (Alcelaphus).—This large genus, despite its number of species, is sharply defined, and though at first sight the caama and tsessebe, bontebok and Hunter's hartebeest, seem very indifferent, yet they possess horns of such a characteristic type, have features and habits so much in common, that it seems a useless multiplication of names to separate this genus into subgenera.

Although to us the caama is the best known species, at once arresting our attention by its ugliness, yet the earliest known kind was the bubaline hartebeest of the north, the bukyel wash, as the Arabs call it. For, on the Egyptian monuments there often appears the figure of an ox with unmistakable hartebeest horns, harnessed to the chariots of the

kings ; and, since all the hartebeests can be readily domesticated when caught young, we conclude that in the days of the Pharoahs they actually broke in the hartebeests as beasts of draught. The Dutch name implies stag-ox, so that the old settlers may have done the same, unless the Zulus brought the Arabic name down with them, and it was then translated by the Boers into equivalent Dutch.

The Caama or true South African hartebeest is, as Cornwallis Harris remarks, made of triangles. The male stands five feet at the withers, and nine in extreme length. The crupper is drooping and the shoulder elevated ; the head heavy, narrow, long. The horns are seated on the summit of a beetling ridge of bone on the forehead, almost touching at the base, thick, diverging and again approaching, turned forewards and then acutely backwards, with points directed horizontally to the rear. The surface of the horns is embossed with five or six prominent knots on the front only. The neck and throat are bare, with no mane. The coat is short and glossy ; color, bright orange-sienna with a crimson cast. There is a black patch at the base of the horns above the forehead, continued behind, and terminating in front of the ear. A black streak down the nose, and a black stripe down the ridge of the neck. Chin black. A black line down the front of each leg, terminating in an angular band above the fetlock. Tail reaching to the hocks, with backwardly directed glossy black hair. Legs slender with taper hoofs. Ears whitish, long, pointed and flexible. A half-muzzle dividing the nostrils ; nose flattened, moist. Eyes high in the head, wild, and of a fiery-red color. Female with more slender horns, and fainter in color. Two mammæ. Young born singly in April and September.

The hartebeest is found in small flocks, headed by three or four stout males, the weaker being expelled and forced to establish a community of their own. In fighting they drop down on their knees, and, placing their forehead parallel with the ground by putting their nose between their legs, butt each other with intense fury, their gnarled and angular horns interlocking, and inflicting gaping, jagged wounds. A common habit is to rake their horns against the trees until they are covered with bark.

In running the caama has long, *oily*, and beautiful paces, which are of the most approved racing style. Moving at a smooth and swinging canter, throwing its hind quarters well under the body, brandishing the glossy tail, and carrying its great beamy head in the best possible manner, it cuts a very majestic appearance, notwithstanding its angular build. So swift of flight is it that the hunter is again and again disappointed when trying to chase it on horseback ; but in and around

Vryburg, and near to the farms in Bechuanaland, it has become so used to the sight of man who protects it, that it no longer regards him as an enemy. Sir C. Harris, however, saw him in a more unsophisticated state, for, he says that, when followed, the caama frequently stops, and turning proudly towards the foe with a most sapient look, sneezes with great violence, an act of overt folly, so much so, indeed, that it would appear to be playing a game of hide-and-seek with the hunter, ever peeping at him from behind the trees.

The flesh is dark and venison-like in appearance, but somewhat tasteless. The skin is in much request among the Bechuanas for karosses.

The caama is very liable to a terrible scourge that affects most of the big game of South Africa. It originates in a kind of bots, probably the larvæ of an Oestrus, which force their way into the nostrils, and the head becomes literally crammed with maggots, numbers of which are expelled in the process of sneezing.

The bontebok and blesbok bear to each other the closest resemblance, being equally robust, with the same hump on the back just behind the neck, the same broad nose, characteristic indeed of the whole Alcelaphine division, and finding its greatest expression in the wildebeests; and, as Harris says, both have the same fine, venerable, old-goatish cast of countenance. The lyrated horns are placed vertically on the summit of the cranium, those of the bontebok being jet black, whereas they are light brown in the blesbok. They have in common the snowy white blase on the nose; the belly is white; and the back hoary and glazed, as though they wore saddles. They are equally addicted to the use of salt, which occurs abundantly in the form of an efflorescence in the Kalahari, and both scour against the wind with their square noses close to the ground, as though they were running scent. The bontebok now survives only on certain farms near Cape Agulhas, but the blesbok has a more northerly range, and formerly existed in great numbers in the Free State and Transvaal.

In the country of the Tamboukies, immediately beyond the eastern frontier of Albany, there exist boundless billowy successions of surge-like undulations, known appropriately as the Bontebok flats; whether the "painted-goat" ever existed there is problematical, but the blesbok used to be shot there in considerable numbers. These two bucks stand out from the rest of the hartebeests by their violet and chocolate coloring; their height is about 3 feet 8 inches, and length 6 feet 4 inches, but animals of this stature are seldom found now.

The sassayby, or, as Livingstone called it, the tsessebe, is found north of Lake Ngami. It stands 4 feet 6 inches at the withers, and some 8

feet 3 inches in length. Horns robust, turning outwards, forming a complete crescent when looked at from before; some 12 or 15 annuli on the lower half, upper half smooth; the characteristic hartebeest zigzag is only faintly reproduced. Selous has noted a hybrid between the tsessebe and the caama. Herr Matchie's *Damalis jimeru* is not clearly separated from the tsessebe. The most characteristic feature of this species is the slate-colored markings on the sides of the shoulders and flanks, while the general color is brown, fulvous or tawny.

Of the North African forms we can only mention here Hunter's hartebeest, which has a much shorter face than the typical caama. It stands some 4 feet at the withers, and is of a uniform chestnut brown, with white tail and belly. A white chevron stretches between the eyes. The horns are inclined outwards at the base, and then run vertically upwards, the greater part being quite smooth; length round curve, 26¼ inches. (The Scientific African, February, 1896.)

Zoological News.—The material obtained by deep-sea dredging in the gulf off the coast of Cape Breton includes many animals hitherto considered as exclusively Mediterranean as to habitat. In view of the importance of this discovery, M. De Folin (de Biarritz) has prepared a catalogue of the species found in the collections, the first installment of which is published in the Revue des Sciences Nat. de l'ouest, April, 1896.

ENTOMOLOGY.[1]

Fossil Cockroaches.—Mr. S. H. Scudder's studies of the American Fossil Cockroaches have recently been published by the U. S. Geological Survey (Bulletin 124). Most of the forms figured and described are from the paleozoic fauna. While, in 1879, only seventeen species of cockroaches belonging to this fauna were known, there are 132 species now described.

Dr. Packard's Monograph of Bombycine Moths.—In the important memoir recently published by the National Academy of Sciences, Dr. A. S. Packard embodies the results of many years work upon the Bombyces. The volume contains about 300 quarto pages and 50 plates, many of the latter being beautifully colored. The scope

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

of the memoir is shown by the following list of contents: Introduction; Evolution of bristles, spines and tubercles of caterpilars; External anatomy of caterpillars; Incongruence between larval and adult characters of Notodontians; Inheritance of characters acquired during the life-time of Lepidopterous larvæ; Geographical distribution; Phylogeny of the Lepidoptera; Classification of the Lepidoptera; Nomenclature of wing veins; Systematic revision of the Notodontidæ.

In classification Dr. Packard adheres to the lines of the paper he recently published in the NATURALIST. The discussion of acquired characters is one of the most interesting parts of the book and is well worth reading by biologists generally. The volume is an extremely notable contribution to the literature of American entomology.

Grape Insects.—Mr. C. L. Marlatt contributes to the recent Year Book of the Department of Agriculture a valuable discussion of the Insect Enemies of the Grape. In the introduction he says: Upward of 200 different insects have already been listed as occurring on the vine of this country, and the records of the Department alone refer to over 100 different insects. Few of these, however, are very serious enemies, being either of rare occurrence or seldom numerous, and for practical purposes the few species considered below include those of real importance. They are the grape phylloxera, the grapevine fidia, both chiefly destructive to the roots; the caneborer, destructive particularly to the young shoots; the leaf-hopper, the flea-beetle, rose-chafer with its allies, and leaf-folder, together with hawk moths and cutworms, damaging foliage, and the grapeberry moth, the principal fruit pest.

The extent of the loss that frequently results from these insects may be understood by reference to a few instances. The phylloxera, when at its worst, has destroyed in France some 2,500,000 acres of vineyards, representing an annual loss in wine products of the value of $150,000,000, and the French Government had expended, up to 1895 in phylloxera work over $4,500,000, and remitted taxes to the amount of $3,000,000 more. The grapevine fidia, on the authority of an Ohio correspondent, in a single season in one vineyard, killed 400 out of 500 strong five-year-old vines. The prominent leaf-defoliators, as the rose-chafer and flea-beetle, frequently destroy or vastly injure the crop over large districts, and the little leaf-hopper, though rarely preventing a partial crop, is so uniformly present and widely distributed as to probably levy a heavier tribute on the grape in this country than any other insect.

Flower-Haunting Diptera.—Mr. G. T. Scott Elliott has made numerous observations which go to show that flower-haunting Diptera are of much importance in pollination. He thinks that his evidence clearly proves the color-sense of the Diptera observed, and also that they "are, on the whole, more intelligent than the lower class Hymenoptera." "It is to these Diptera," he says, "that we probably owe all of the neatly made, small and bright colored forms of flowers." The author gives tables showing the visits of about sixteen Diptera to various types of flowers, and compares these with the visits paid by Hymenoptera. He suggests that the Diptera map out the ground as vultures do, and keep flying up and down over a chosen area. At the beginning of his paper,[2] there is an interesting note on the part which insects play in *isolation*. Thus if flowers of the same species occur partly inside a sheltered wood, and partly outside, probably not more than five per cent of those outside will be fertilized by pollen from those inside the wood and *vice-versa*. This means for reproduction almost perfect isolation.—*Journal Royal Micr. Society.*

Larval Habits in Panorpa.—Dr. E. P. Feldt contributes to the tenth report of the State Entomologist of New York an important paper on Scorpion fleas from which we quote the following relating to *Panorpa rufescens:*

"Throughout their different stages, the larvæ usually harmonize with their surroundings so closely that it is difficult to detect them. Frequently a slight motion of the earth is the first indication of their presence. They burrow in the earth and remain underground much of the time. Many burrows ran less than one inch below the surface, although a few extended to a depth of three or four inches. The larvæ may be fed readily upon raw meat placed upon the surface of the ground. Some time after placing the meat in the cage, they may be found under it, frequently in a more or less cell-like depression. When in such a position they rarely try to escape, but trust to their protective resemblances, and remain motionless. Around the edge of the piece of meat and also under it, the mouths of burrows may be seen and in them the heads of larvæ; when in such positions they dodge back quickly at the least disturbance. Unless the meat is moved very cautiously the burrows will appear empty; but if quiet is maintained for a few moments, the heads will soon be seen. The burrows opening under the meat frequently come to the surface a little distance away, and it is quite easy to drive a larva out of its back door. Not infre-

[2] Trans. Ent. Soc. London, 1896, pp. 117–118.

quently they have been observed to emerge from a burrow for their feeding. This usually occurred in the afternoon. On one of these occasions a little fellow was watched through a simple lens. It was interesting to see him bite off a piece of meat and swallow it with every evidence of satisfaction. The antennæ were moved back and forth in the most appreciative way. As the larvæ increase in size, more burrows open upon the surface and they are seen lying at their mouths. One time two were seen out of adjacent burrows. The larger seized the smaller in the back and tried to drag it down into its burrow. The smaller was unable to escape, and when it was pulled away with forceps the body-wall was ruptured. At another time a smaller active larva was seen to attack a larger inactive one, which, unable to resist, was bitten so severely that the segment swelled considerably, but was not ruptured. In a day or two the larger died and was fed upon by its former persecutor.

EMBRYOLOGY.[1]

The Wrinkling of Frog's Eggs During Segmentation.—The occurrence of wrinkles in frog's eggs during the process of segmentation was first observed and very briefly described by Prevost and Dumas, who have the honor of being the first observers of the segmentation itself (Annales des Sciences Naturelles, I ser., 1824, Tom II, p. 110).

A somewhat better description was given later by Bär (Archiv. für Anatomie, etc., 1837) and Reichert (the same, 1841), who gave to the phenomenon the name "Faltenkranz," and made some attempt to explain its nature and origin.

By far the best description, and, indeed, the only really good one which has ever been published, is that by M. Schultze, which appeared in 1863 (Observationes nonnullæ de ovorum ranarum segmentatione, Bonnæ).

He gives an excellent account of wrinkles observed in the eggs of *Rana temporaria* and *R. esculenta*, and concludes with an "explanation of their origin." But he really devotes only a very few lines to the explanation, and gives up the remainder of this portion of his paper to

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

a controversy with Reichert, over the existence of a vitelline membrane on the surface of the egg.

Similar wrinkles have since been observed in the eggs of the common toad by Goette (Die Entwickelungsgeschichte der Unke), and of *Amblystoma punctatum* by Eycleshymer (Journal of Morphology, Vol X). But neither of these authors offer any explanation of their origin. So far as known this "Faltenkranz" has never been described for any American species of frog, nor has any attempt been made to study it by means of serial sections.

The following paper is offered as a contribution of some new and interesting details in the occurrence of the phenomenon, together with the results of a microscopic study of sections, in the hope of arriving at a rational conclusion as to its origin.

The author desires to acknowledge his great indebtedness to Dr. E. A. Andrews, for the suggestion which led to the study, and for much subsequent assistance. Thanks are also due to Prof. T. H. Morgan, for the kindly loan of a copy of Schultze's paper.

FORMATION OF THE WRINKLES.

The eggs of a small wood-frog, in all probability *Chorophilus triseriatus*, were obtained for class work on March 27, 1896.

They were unsegmented when found, and were immediately placed in ice water to check any further development.

After remaining thus for five hours they were used in the laboratory, being removed to watch-glasses containing tepid water. Some were allowed to remain in the ice water for eight hours before being examined, and it was noticed that these segmented much more rapidly than the ones which had been kept only five hours. Actual segmentation had been prevented during the stay in the ice water, but there seemed to have been a storing-up of energy, a sort of preparation for segmentation, so that when removed to a favorable environment the process began very quickly (5–10 mins.) and was carried on much faster than it would have been normally.

These eggs were obtained from pools covered with ice quarter of an inch thick, and most of the bunches were quite near the surface.

This must occur frequently where the eggs are laid so early in the spring; and according to Morgan (AMERICAN NATURALIST, August, 1891), Chorophilus always lays its eggs very early.

The storage of energy noticed above may suggest a natural method of compensation whereby the warm sunshine of mid-day may offset the freezing cold of the nights, and in this way the eggs will really lose very little time in their development.

O. Hertwig and Schultze have recently experimented on the influence of a very low temperature upon the development of the eggs of *Rana fusca* with very different results.

Hertwig (Sitzungsberichte der König, Preuss. Akad. d. Wiss, 5 April, 1894, p. 313) found that freshly fertilized eggs were injured by an exposure to a temperature of 0° C. for 24 hours. On being raised to the ordinary temperature a portion were developed very much more slowly than normal eggs, while in the remainder a part of the yolk was found incapable of division.

Schultze (Anatomischer Anzeiger, X Band, No. 9) subjected eggs of the same species to a temperature of 0° C. for 14 days, and then obtained perfectly normal embryos from them. These eggs had reached later stages of development before being cooled, and he does not state whether their subsequent development was more or less rapid than ordinary.

Subjection to a temperature of 0° C. for so long a period would probably have a very different effect from that of only a few hours duration.

Loeb and Norman (Archiv. f. Entwick, III Band, No. 1), experimenting on the eggs of the sea-urchin, Arbacia, found that when put in sea water to which had been added 2–3 per cent. NaCl or $MgCl_2$ segmentation of the protoplasm was wholly prevented, but that the nucleus went on dividing. On being put into normal sea water after a few hours exposure to the concentrated solution the eggs divided at once into several cells, the protoplasm merely rearranging itself around the new nuclear centers.

Possibly the same may be true in these frog's eggs when subjected for a short time to a freezing temperature, and the subsequent hastening of segmentation may be due to the fact that the nucleus has already divided.

In watching the segmentation of these eggs the great (comparative) size and depth of the furrows was specially noticed, together with the distinctness of the wrinkles formed along either side of them.

Accordingly it was determined to study their segmentation more in detail, and a fresh lot was obtained the next morning. These remained in ice water only one hour, just long enough to get them home. They were then transferred to watch-glasses and examined in strong sunlight. This fact must be taken into account in connection with the time periods given.

The first furrow appears at the superior pole without any previous flattening as in Amblystoma (Eycleshymer, Jour. of Morph., X, p. 348). At first it is a shallow groove just at the pole itself, but it soon spreads

over the pigmented hemisphere. The ends may progress at a uniform rate, or one may exceed the other in rapidity as in Petromyzon (Eycleshymer, loc. cit.).

At the first appearance of the furrow it is very shallow and perfectly smooth, and extends about 0.2 mm. on either side of the pole. It begins to deepen in two or three minutes, and at the same time minute wrinkles appear on either side (Plate I, figs. 1, 5, 7). If the eggs be placed upon a black surface in the sunlight these wrinkles are seen very distinctly as fine lines radiating from the pole (Plate II, fig. 31). The number and arrangement of these lines is not constant or definite either at this, or any subsequent period. The suggestion at once presents itself, that these wrinkles may be the foreshadowing of subsequent segmentations, and one egg was obtained in which the wrinkles seemed especially significant in their arrangement (Plate II, fig. 32). The entire lack of regularity in the size and arrangement of the wrinkles would, however, preclude any such idea, since the subsequent segmentations are very regular.

As the furrow gradually deepens and extends toward the yolk two changes are noticed :

1. New wrinkles appear along its sides ; these do not all radiate from the pole, but are inclined toward it at greater or smaller angles (Figs. 6, 8, 11).

2. The radial wrinkles first formed at the pole change considerably. Very fine and delicate at first they coalesce gradually into a few larger and deeper ones, which are sharply defined.

These fused ones may or may not occupy the position of one of the antecedent ones (Figs. 2, 3, 33, 34, 35). As the furrow progresses the number and position of the wrinkles changes constantly, new ones being formed and old ones disappearing. This is especially true of the finer ones ; some of the larger fused ones near the pole remain quite constant (Figs. 10–15).

This continual changing is best seen by making sketches of the wrinkles with a camera lucida at short intervals, as in the movements of the pseudopodia of Amœba (Figs. 1–4 and 10–15).

The whole appearance thus far is exactly as if the egg were covered by a very thin, but firm membrane, which was gradually pulled in toward the center at the groove. The remainder of the sphere being perfectly even, and with no chance for "give" at any point, in consequence of the uniform tension, the edges of the groove would necessarily be wrinkled, the wrinkles becoming more and more prominent as the groove deepened. The whole process takes place so slowly that the

most careful scrutiny fails to detect the actual motion, or to see evidences of any movement of the protoplasm within the egg, which might cause the wrinkling.

The furrow at the pole has become quite deep by the time its ends have reached the equator, in five or six minutes. The ends seem to stop here for a time, just at the border of the yolk area, while further changes take place in the pigmented portion. First the two edges of the groove approach each other at the pole, and seem to fuse slowly, the wrinkles entirely disappearing during the process. This fusion then extends in either direction along the groove for some distance, often half way to the equator, obliterating the wrinkles as it goes. By this means the groove may entirely disappear at the center while remaining near the periphery (Fig. 38).

At this point the furrow begins to enter the yolk area on either side, and at the same time the groove reappears along the center of the pigmented hemisphere. It now becomes very broad and deep. Indeed, it seems to reach clear through to the yolk, and its walls are considerably rounded on either side. But they are now smooth, so that the wrinkles remain in all fifteen or twenty minutes on the first furrow.

This first furrow divides the egg into two nearly equal parts (Figs. 3, 12, 28, 35). When its ends first reach the yolk area, where they stop for a time, as already noted, the two blastomeres thus formed are very much rounded at their ends, and diverge strongly from the groove. This is readily seen in the series given in figs. 1 to 4, but it becomes much more prominent after the reappearance of the groove—(Figs. 28 to 30). Under a higher magnifying power a wide space can now be seen at either end between the two segments. The floor of this space is triangular in shape and doubly curved, being concave from side to side, and convex antero-posteriorly. It is formed of light colored yolk, into which the pigment shades gradually around the borders. The two segments are thus rounded in a manner very similar to that of the first two blastomeres of a meroblastic egg.

As the furrow proceeds toward the inferior pole the space between the two pigmented segments diminishes, the borders of the furrow approach each other, and the surface of the egg becomes smooth once more, with the groove indicated merely by a narrow, faint line.

It remains in this condition some eight or ten minutes before the second cleavage begins, and this may be called its resting stage. The rapid closing of the groove previous to the appearance of the second furrow occurs also in Amblystoma (Eycleshymer).

The Second Cleavage.—The second furrows begin about forty or forty-five minutes after the first. They start *from the equator* and move toward the dark pole, and not vice versa as in Rana, Amblystoma, Petromyzon, and most of the amphibians. This was without exception in all cases noted (some thirty-five or forty). They form a right angle with the first groove, but do not always meet it at the same point (Figs. 19, 20). They are attended with the same appearance, fusion, and gradual elimination of wrinkles as the first furrow, with the slight difference that the wrinkles start at the equator instead of the pole, and are never quite as large. This is shown in figs. 16 to 18, in which both the wrinkles and the furrows can be seen approaching the pole.

The second furrows seem to reach the first over the dark hemisphere before they start toward the inferior pole. They start at about, but not necessarily exactly, the same time.

Here, too, there is the same rounding of the pigmented segments, though to a much less degree. This may be due to the fact that in this instance the furrows start from the periphery, and consequently the segments are more sharply defined there.

The Third Cleavage.—Half an hour after the appearance of the second set of furrows, and sometimes before they have reached the inferior pole, there are indications of the first horizontal cleavage. As seen from figs. 21–24 this occurs very much nearer the superior pole than the inferior. The furrows, accompanied as before by wrinkles along their edges, as seen in the figures may start in each segment either from the first or second vertical, usually, however, from the second (Fig. 22).

They move much more rapidly than the preceding cleavages, and the entire process is completed in fifteen to eighteen minutes.

In all the eggs observed the grooves started usually at the second vertical in the two quadrants on the same side of the latter, and moved around toward each other at the first vertical.

They seldom met at the same point, but as a result of their formation the superior region was divided into four quite equal and nicely rounded cells, much smaller than the four inferior ones.

The only wrinkles formed upon the yolk area that could be detected by the most careful examination are the few which occur on the yolk side of this first horizontal groove. Schultze failed to detect any wrinkles whatever along this horizontal furrow, although he describes the other details with great exactness. This is all the more striking, because he both observed and figured them upon subsequent furrows up to the

32-cell stage. Other authors, with possibly a single exception, make no mention of them, except along the first furrow.

This confining of the wrinkles almost exclusively to the pigmented area is manifestly connected with the different organization of the two halves of the egg. The pigmented half is richer in protoplasm, and is to a higher degree under the influence of the cell nucleus; while the yolk has its protoplasm scattered about amongst the yolk granules, and is also further removed from the nucleus which lies in the pigmented half in the undivided egg (Hertwig). This results in the more rapid segmentation of the pigmented cells, and the presence of the wrinkles seems intimately associated with this rapidity of segmentation.

The Fourth Cleavage.—This appears from fifteen to twenty minutes after the third.

In this cleavage also the furrows started in every instance *from the periphery* of the four superior quadrants and move toward the pole, accompanied by wrinkles. These latter are now much smaller than heretofore, and are not easily detected under a low power. They are also very transitory, and disappear almost immediately. There is somewhat of a tendency in these furrows to run nearly parallel to the first or second vertical, recalling the conditions in teleosts (Figs. 26, 27).

There is a subsequent fusion and elimination of the furrows after each cleavage, as has already been noted in the first segmentation.

This elimination of the grooves, due to the fourth cleavage, leaves the pigmented pole of the egg divided into four cells of a totally different shape and arrangement from that of the four blastomeres resulting from the third cleavage (Fig. 37). After remaining thus for several minutes the furrows reappear, and the cells resume the shape seen in fig. 36.

From this point segmentation proceeds very rapidly, and in a manner exactly similar to that of other frog's eggs. The wrinkles have now become so small as to be seen only with the greatest difficulty and under a high power, and they disappear so quickly as to easily escape detection. But they are present at least up to the 128-cell stage, and appear, fuse, and disappear, as in the first cleavage.

Gastrulation begins about twelve hours after the first cleavage, and the blastopore closes at about the fifteenth hour. The neural folds appear and gradually fuse to form the neural canal as in Rana. By the end of the first day the embryo has elongated considerably, and the head is well differentiated.

The tail then becomes defined, the gill folds appear, and the eyes are seen as two minute black spots. And by the end of the third day the embryo escapes from the egg envelopes and swims about freely.

The yolk sac seems unusually large, and the tail is comparatively long at this period, but otherwise these tadpoles are externally like those of Rana and Hyla.

NATURE AND ORIGIN OF THE WRINKLES.

If the eggs be preserved during the first segmentation while the wrinkles are still present, and then sectioned parallel to a plane tangential to the superior pole, considerable additional light is thrown upon this process of wrinkling.

As has been noted in the eggs of other frogs the pigment is gathered into a thin surface layer over the superior hemisphere.

Houssay states that the pigment does not characterize this pole, but only happens to be there on account of the coincidence of its density with that of the surrounding protoplasm (Etudes d'Embryologie sur les Vertébrés, Archiv. de Zoöl. Exper., 1890). However this may be, during segmentation pigment appears along the sides of the furrows, so that eventually the resultant cells come to have a more or less definite pigment layer around their periphery, Houssay accounts for this by saying that there is an intimate relation between the activity of the cell and the presence of pigment. When the resting cell becomes active its granules become smaller, and pigment appears in them as the result of chemical action.

The presence of pigment, therefore, is the result of an increased activity in the cell.

But Bambeke tells us that "the cortical layer, when it enters the interior of the protoplasm, is not entirely employed in limiting the spheres of new formation. In fact, I find masses of pigment whose presence can only be explained by considering them as debris from the cortical layer, which has penetrated into the protoplasm" (Fractionnement de l'Oeuf des Batraciens, Archiv. de Biologie, Vol. I, p. 346, footnote).

An examination of sections of these Chorophilus eggs shows a similar occurrence.

In addition to the pigment layer which borders the first segmentation furrow, and which is somewhat thicker near the centre of the section, there is also a lunate mass extending downward vertically from the superior pole on either side of the furrow (Fig. 39) and in immediate contact with it. This mass can be traced in the sections from the surface layer, in which it has very little area, down somewhat beyond the bottom of the furrow, where it spreads out laterally and is lost in the surrounding protoplasm. In this particular egg the mass does not

extend quite to the level of the nuclei, the position of the latter in the figure having been taken from subsequent sections in the series. This mass, however, is not to be regarded as "debris," but its presence is due to a definite cause to be explained later. The remainder of the section is occupied by homogeneous protoplasm filled with rather small yolk granules, and surrounded by the thin, transparent vitelline membrane.

Under a high power (Fig. 40) the wrinkles appear as deep sinuses extending obliquely into the protoplasm, and bordered by a thick layer of pigment. These sinuses are angular and irregular in outline, and often cantract at their inner ends into long, narrow slits, with rather distinct walls. We can now see what it was impossible to detect from a surface view, namely, that the wrinkles are compound.

The larger, principal ones have secondary, smaller ones extending outward from their sides, approximately at right angles. Schultze observed and figured these compound wrinkles in his surface views of R. temporaria, and adds another detail which I have been unable to find in the Chorophilus eggs, viz., the breaking-up of a single wrinkle at its peripheral end into several smaller ones arranged radially from a common point.

The pigment usually fills the projecting protoplasm between adjacent sinuses. It is also much thicker in the region of the wrinkles than elsewhere along the furrow, as can be seen in fig. 40.

In view of these different facts, therefore, it seems evident that there is an intimate relation between the wrinkles and the pigment—and that both may be results of the same cause. It remains to ascertain what this cause is, if possible.

According to Schultze the egg is a single cell, and just as cellular division is brought about by the contractility of protoplasm, so is the segmentation of the egg due to the same cause. These contractions originate at the point where the furrow begins, and are at first confined to a very small area. Since the cortical portion of the egg protoplasm possesses a glutinous consistency, it is not to be wondered at that folds or wrinkles appear at the same time with the furrow, in its immediate vicinity, and at right angles to it. These subsequently disappear in consequence of the difference in contractility between the outer and inner protoplasm, due to their different consistency.

It is exceedingly difficult to understand how compound wrinkles, of such a nature as we have just described, could be produced by the simple contraction of a viscous cortical layer of protoplasm, especially if that contraction *starts from a fixed* point in the layer. Indeed, how

could it produce any wrinkles at all in the layer itself? Would it not rather tend to flatten the superior pole and stretch the viscous layer tightly over the underlying protoplasm in a manner similar to the action of the muscles of the diaphragm during respiration?

As to the difference in contractility between the outer and inner protoplasm, it is evident that if this is to smooth out the wrinkles, the difference must be in favor of the outside layer, and also the contraction must be *at right angles to the length of the wrinkles.* That means in the present instance that it must be parallel to the first furrow. Neither of these conditions seem possible, and we thus find both explanations inadequate when confronted by the facts in the case. They both fail to account for the mass of pigment under the superior pole also.

Bambeke states that "the entrance of cortical pigmented masses into the interior of the egg . . . can only be explained by admitting the existence of contractions in the ovular protoplasm during segmentation."

According to the interpretation of the present day, cell division is brought about by means of some force or forces acting along the length of the segmentation spindle. In the present instance this spindle was formed between the two nuclei represented in fig. 39, which lie some distance below the surface of the egg.

If we interpret Bambeke's "contraction of the ovular protoplasm" to be identical with this working force of the segmentation spindle, it will explain the presence of the lunate mass of pigment directly over the spindle, and will help us to understand the presence of a pigmented layer on either side of the segmentation furrows. But it does not explain in any way the formation of the wrinkles.

Reichert's wonderfully inconsistent explanation of the origin of the wrinkles is quoted, and sufficiently commented upon by Schultze, in the paper already referred to.

In view of the fact, therefore, that we have no explanation which can stand the test of our present knowledge of cell division, we venture to offer the following:

We agree with Schultze that the external pigmented layer is necessarily somewhat denser that the internal protoplasm.

Modern research indicates that this layer is drawn inward in some way, by the forces working along the segmentation spindle, to form the furrow which lies over the equator of the spindle. The bottom of the furrow, thus formed by an infolding of the surface layer, describes an arc which becomes shorter and shorter as the furrow deepens.

This shortening of the arc must result in one of two things. The bottom of the furrow may remain of the same length as at first, and

make up for the shortening of the arc by protruding a little from the surface of the egg at either end of the furrow. Such a condition is admirably shown when one creases the top of a soft felt hat, and would necessarily be even more manifest if the hat were filled with a viscous fluid.

An examination of any of the surface views will show that this is not the case with these Chorophilus eggs, but that the first indication of the groove at either extremity is a slight hollowing in of the surface, and not a bulging outward. This view is confirmed by a study of the sections.

The other alternative is that the shortening of the arc must result in longitudinal condensation along the bottom of the furrow, starting at the center and increasing as the furrow progresses.

Such a shortening or contraction along the bottom of the furrow would very naturally throw its sides into folds or wrinkles at right angles to its length. The pigment layer in contact with these walls would also be thickened in the region of the wrinkles, and toward the center of the groove.

Since the condensation starts at the center and advances in both directions with the progress of the furrow, the wrinkles would be arranged somewhat radially about the superior pole. This progressive contraction also accounts for the successive appearance and disappearance of wrinkles, and for the confluence of smaller into larger ones.

As the sides of the furrow begin to fuse into the permanent segmentation plane or cell-wall the wrinkles disappear through the gradual readjustment of former relations.

Indeed, the whole phenomenon seems very largely dependent on the rapidity of segmentation and the consequent sudden disturbance of normal relations before the different portions can adjust themselves to their new conditions.

This fact will serve to explain why the wrinkles show so prominently in this particular species, which has a very rapid development, and also why the conditions under which they were examined—the transference from ice to tepid water and the placing of the eggs in strong sunlight—were especially favorable.

It may also suggest a reason why one observer has failed to detect wrinkles in the eggs of a given species, while another, working under more favorable conditions, has seen and described them. And it will in a measure account for the absence of wrinkles on the yolk hemisphere, since segmentation is very much slower there.

If our explanation is a correct one there ought to be no wrinkles at all along the bottom of the groove, while they should be present and have their greatest depth about half way between the bottom and the surface.

The sections show that this actually occurs. Fig. 39 is a section cut just at the level of the bottom of the groove, and shows no trace of any wrinkles, nor are there any in the two or three preceding sections.

They then appear and gradually increase in size up to the level of fig. 40, which is a magnified portion of the same groove about half way to the surface.

The problem of the compound nature of the wrinkles finds its solution in the fact that there must be a condensation along the bottom of the larger wrinkles, in all respects similar to that in the groove, and due to the same cause, though, of course, on a very much smaller scale.

But in this instance the condensation would proceed in only one direction, and hence we find the secondary wrinkles all inclined in the same direction to the principal ones, just as we have already observed, and as Schultze has so finely figured.

Summary.—1. Subjection to a temperature of 0° C. for a period of eight hours completely arrests all development for the time being, but results, on the subsequent restoration of ordinary conditions, in a cleavage more rapid than that of normal eggs.

2. Segmentation, at least up to the 128-cell stage, is accompanied by the formation, fusion and subsequent elimination of well defined wrinkles along the sides of the furrows in the pigmented area. There are no wrinkles on the yolk, except along the inferior border of the third cleavage furrow.

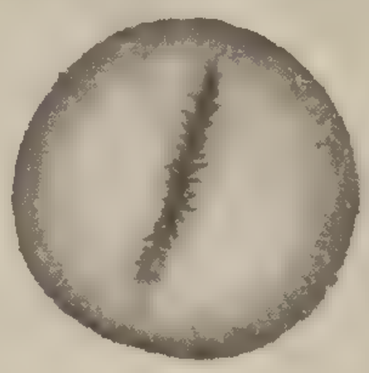
3. As seen in an examination of cross-sections these wrinkles are compound in nature, the larger, principal ones having smaller secondary ones along their sides.

4. The wrinkles on the first furrow are arranged somewhat radially about the superior pole. On subsequent furrows they are inclined at an angle toward the point where the furrow starts.

5. The pigment which borders the segmentation furrows forms a thicker layer in the region of the wrinkles than elsewhere along the groove, thus showing an intimate relation between the two.

6. The probable cause of the wrinkling is to be found in the condensation along the bottom of the groove, which results from the shortening of the arc, and is a necessary consequence of the infolding of the surface layer to form the groove.

PLATE XIII.



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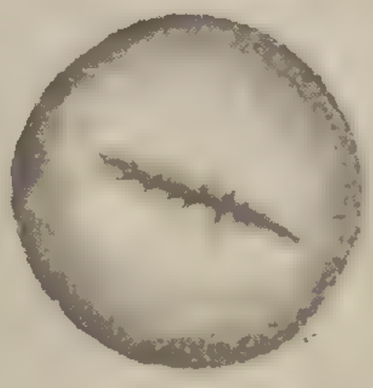

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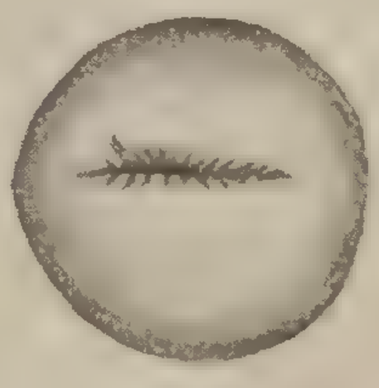

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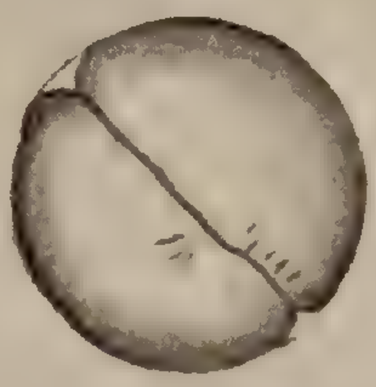

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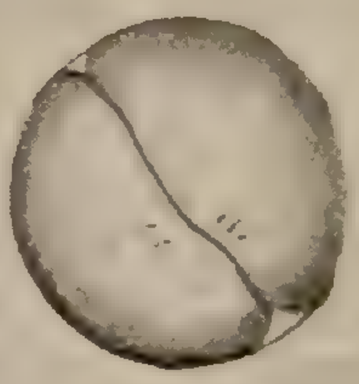

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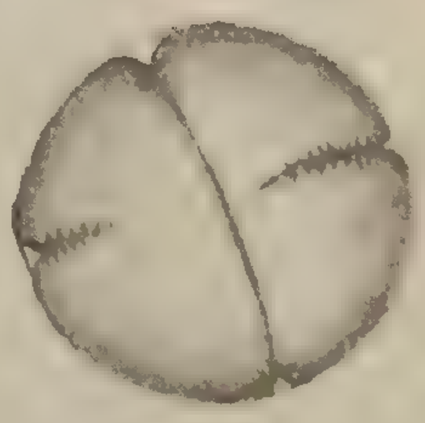

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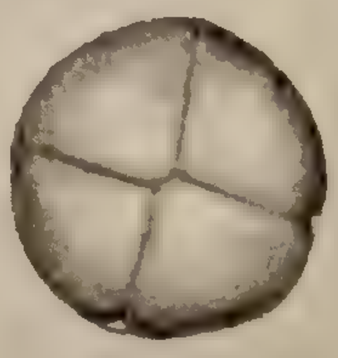

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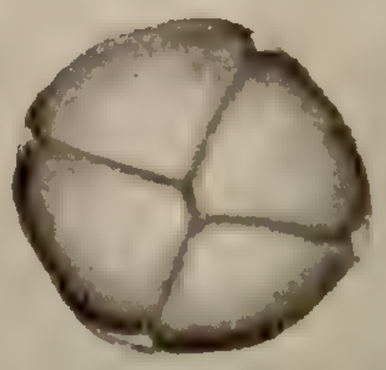

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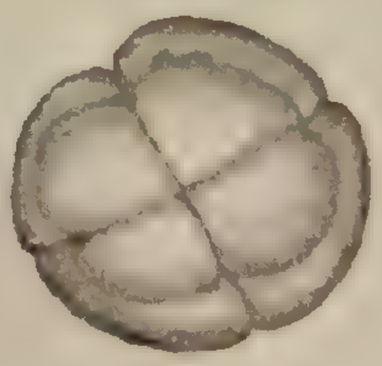

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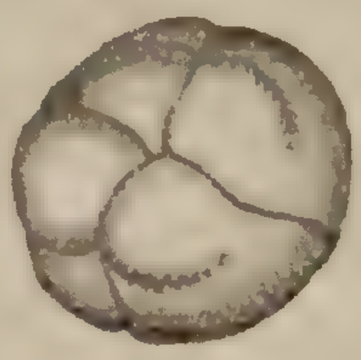

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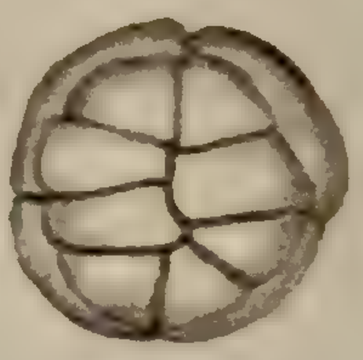

27.

Segmentation of Rana.

PLATE XIV.

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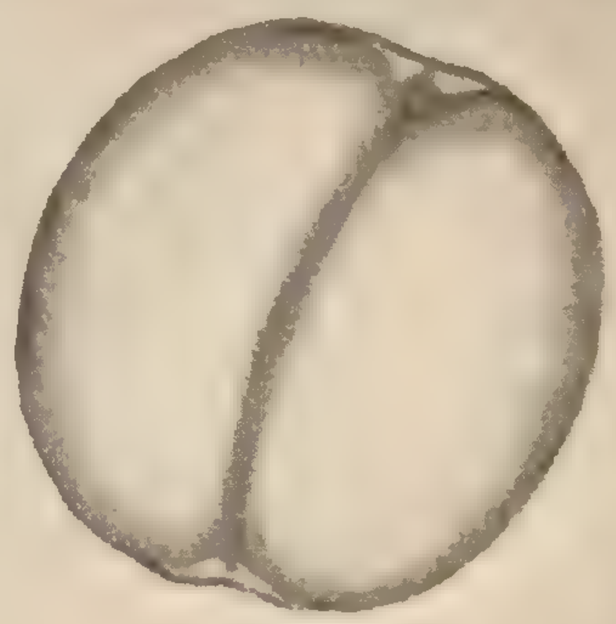

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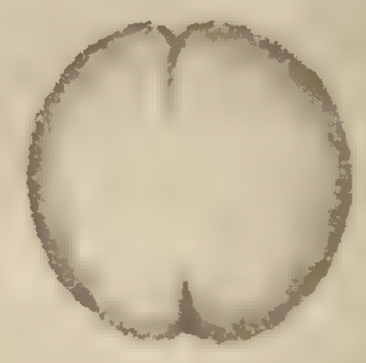

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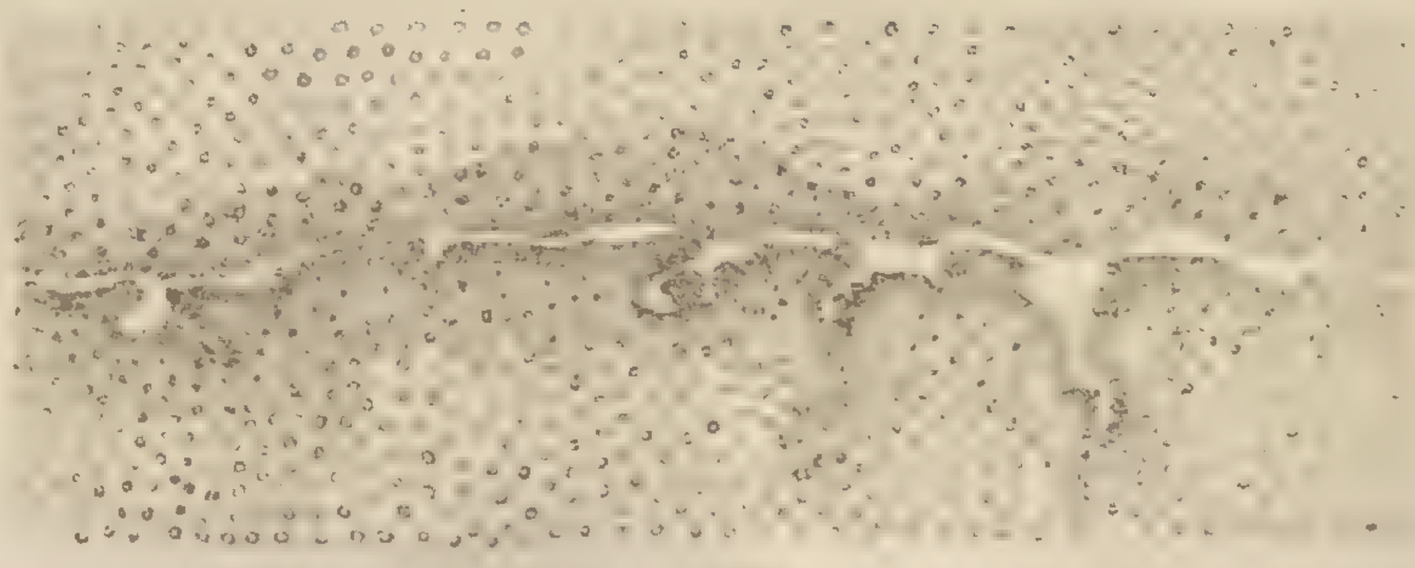

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Segmentation of Rana.

7. The second and fourth grooves *start from the periphery* and move toward the pole.

8. The blastomeres are more rounded and the segmentation furrows are deeper than those in most frog's eggs.

9. The development is very rapid; gastrulation begins within twelve hours, and the tadpole escapes from the egg during the second or third day.—CHARLES B. WILSON.

EXPLANATION OF PLATE I.

All figures drawn with Zeiss Camera x 16 diam.

Figs. 1–4. Successive stages in cleavage of living egg at intervals of 3 mins., 3 mins., 2 mins.

Figs. 5, 6. Stages in cleavage of second living egg.

Figs. 7–9. Stages in cleavage of third living egg—intervals, 2 mins., 4 mins.

Figs. 10–15. Stages in cleavage of fourth living egg—intervals, 2, 6, 3, 4, and 5 mins.

Figs. 16–18. Stages in second cleavage of third egg.

Figs. 19, 20. Variations in second cleavage.

Fig. 21. Eight-cell stage of first egg.

Figs. 22–24. Stages in third cleavage of second egg.

Figs. 25–27. Variations in sixteen-cell stage.

EXPLANATION OF PLATE II.

All figures drawn with Zeiss Camera.

Figs. 28–30. First cleavage under higher magnification to show rounded blastomeres.

Figs. 31, 32. Beginning of first cleavage, showing radiating wrinkles at pigmented pole.

Figs. 33–35. Variations in wrinkles on the first furrow.

Fig. 36. View of an egg during the fourth cleavage.

Fig. 37. The same egg four minutes later.

Fig. 38. Fusion and partial disappearance of the first groove.

Fig. 39. Horizontal section of an egg during the first segmentation, taken at the level of the bottom of the furrow. Nuclei added from the fourth section below this.

Fig. 40. A portion of the same furrow about half way between its bottom and the surface of the egg, more highly magnified.

PSYCHOLOGY.[1]

Fear Among Children.—The term *fear* is applied with some ambiguity to two distinct phenomena. The sudden and unexpected advent of danger arouses the whole organism and causes an uncontrollable excitement which manifests itself in violent agitation, momentary paralysis, or other well-known signs. While the strength and duration of the emotion depends largely upon the temperament and nervous condition of the individual, it is primarily a function of the immediate stimulus, and its basis is physical rather than mental. Chronic fear, on the other hand, is determined more by the constitution of the individual than by external stimuli, and remains present after the circumstances which called it forth are removed. It occurs in every degree, from the purely normal to the extreme pathological, as exhibited in certain forms of insanity. The normal phases of this emotion are best observed in children, where repressive self-control is less liable to interfere with its open manifestation.

Prof. Alfred Binet has recently carried out such an investigation.[2] He issued a questionary, addressed principally to school-teachers, but circulated also among parents who seemed qualified to give discriminating answers. From the nature of the case, the replies dealt generally with the more permanent form of fear (*peur* as distinguished from *crainte*). On examination of the reports, M. Binet classes the causes of fear as follows: 1. Night, darkness, solitude—the sense of mystery in things—in short, what might be termed in English the quality of *phantomhood*. 2. Loud noises, such as thunder or the report of a gun. 3. Objects which excite repugnance or disgust: small creatures, such as rats or spiders; the sight of blood or a corpse. 4. A danger, real but not hitherto experienced by the child, whose likelihood is greatly exaggerated and which preys upon his mind; thus a child may be afraid of meeting a beggar or a drunken man, of being bitten by a dog, etc. Such a feeling is generally traceable to some story, true or false, which the child has heard. 5. The memory of a severe accident or narrow escape leads to a chronic fear of its recurrence.

We may carry the analysis a step further than Prof. Binet. In 2 and 5, the distinctive element is a sudden nervous shock, with its after-

[1] Edited by H. C. Warren, Princeton University, Princeton, N. J.

[2] La peur chez les enfants, *Année psychologique*, II, 223.

effects; in 3 and 4, the influence of the imagination predominates. Although the imagination is an important factor in 5, this class presents a distinct problem of its own, deeper rooted, which may lead the inquirer into the sphere of comparative psychology.

A point in the investigation most difficult to ascertain, was the proportion of children susceptible to fear. The answers returned varied widely, probably because the distinction between the two kinds of fear was not usually taken into account by the observers. The most reliable data seemed to indicate a general average of about 10 per cent; but the proportion appeared to be at least three times greater among girls than among boys. The question of temperament was investigated, but here too the answers varied considerably, and most diverse traits were included in the different lists; the only generalization that seemed warranted was the preponderance of fear among the gentle and timid,—which is, after all, not a point of startling novelty. On the intellectual side, where teachers are in a position to give trustworthy judgments, the figures indicate a slight excess of fear among the brighter, and a lower proportion among the more stupid than among the mediocre. Prof. Binet argues, however, that this is not due to a direct connection, but that the tendency to fear is increased by a vivid imagination, which is generally associated with greater intellectual capacity. On the other hand, there is a close connection between fear and the state of the health; and a nervous condition, whether due to a shock or otherwise, is fruitful soil for fear in children as in adults. But a further element must be reckoned with here, in the case of the child: for, as he grows conscious of his failing, he loses confidence in himself, and thereby becomes still more liable to fear.

Aside from the concrete causes of fear already noticed, a number of factors are concerned in its development. Heredity plays a prominent part here as elsewhere. Ill-treatment is a most effective agent in fostering it, and this heading may be extended to include the many instances of misdirected efforts to train the child which are far from wilful. The pedagogical value of the study, which M. Binet brings out in a closing section, is nowhere more marked than here. Closely allied to this factor is the influence of tragic stories and mysterious tales on the child's imagination, a principle which even judicious parents are apt to forget. Finally, the force of example—the *contagion of fear*—is shown unmistakably by Prof. Binet's study. The younger is influenced by the older, the stronger by the weaker, the child by the teacher; if the latter show signs of fear in any crisis, the former is

almost sure to give way. This is, of course, no new discovery, but it is a fact which cannot too often be emphasized.

Fear begins to be manifested between the second and third years of age, and, until about the ninth year, the child's powers of self-control are insufficient to inhibit it. Under normal conditions, it decreases rapidly from the ninth until the twelfth year, when, apart from the influence of special conditions or circumstances, it comes well under control.—H. C. Warren.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

American Association for the Advancement of Science.—This body met at Buffalo, New York, from August 24th to 29th inclusive. The council met on the 22d, and the 29th was devoted to an excursion to Niagara. The attendance was not as large as sometimes, the number of members present being 333. The quality of the papers was said to have been in general excellent. Only three of the sections continued in session on Friday afternoon (the 28th), viz.: the Geological, the Anthropological, and that of Social and Economic Science. The affiliated societies coöperated to a considerable extent, the Geological and Chemical Societies reading papers in the appropriate sections, and the Entomological Club suspending its meeting. Eighty-two fellows were elected. Prof. Wolcott Gibbs was elected an honorary fellow, and Mr. Horatio Hale a fellow for life. On Wednesday afternoon a symposium was held in the Geological Section in honor of the sixtieth anniversary of the appointment of Professor James Hall to the position of Director of the Geological Survey of New York.

At the opening session the association was welcomed by addresses from Mayor Jewett, and from Dr. Park, President of the Society of Natural Sciences. President E. D. Cope replied in the following language:

"*Mr. Mayor, Ladies and Gentlemen of the Local Committee and Citizens of Buffalo:* I utter the sentiments of the American Association for the Advancement of Science in expressing our pleasure at being once again in your beautiful city. We feel at home here, and we know that we are among friends who understand our motives and our objects. But, inasmuch as we represent the entire nation, I will give a brief outline of the objects of the Association, and the aims which it has in view. Our principal occupation is that of original scientific research, although

many of us are of necessity also teachers of scientific knowledge. The primary object of the Association is, however, not teaching, but the advancement of science by the increase of knowledge. We seek to penetrate the unknown and to build up a system which will express with certainty the mutual relations of the various parts of the universe, including ourselves. Although many facts are known, and some laws have been formulated, very much still remains unknown, and many of the highest principles of nature remain undiscovered. Original research furnishes the material for teaching and the matter which is contained in books. Much money is devoted to the building of libraries and of schools, but not much is given for the purpose of supplying the knowledge which is to be taught in the schools and from which books are made.

"The motives of the original investigator vary with his years, but the taste for research is generally developed early in life. In some it is a love of the beautiful, whether it be the beauty of a perfect mechanism or the beauty of form that attracts him. In some, it is the desire to know, and in others it is a high interest in the problem of human origin and destiny. In many it is the same feeling which prompts the adventurous explorer to enter new regions, not knowing what he will find, but believing that whatever is, is right.

"The services rendered by science are twofold. They have a value either material in their character or utilitarian, or they have a mental value, inasmuch as knowledge serves to clear the mind of fears and doubts, and so to promote human happiness. The true man of science is not influenced by utilitarian considerations, but pursues the truth wherever it may lead, knowing by experience that its benefits are many and sometimes unexpected. Another benefit which the cultivation of science promotes is the formation of correct habits of thought. The rational faculty of the mind is of very ancient origin, and developed early in the history of man. But its use in the early stages of human development has been largely *a priori;* that is, in the advance of knowledge, rather than as a digestor of knowledge after its acquisition. In other words, the scientific method consists not in the use of abstract reason, but in a rational use of the results of observation and experiment. This is the lesson which the history of science teaches mankind, that if we wish to know the actual state of affairs, our course is first to observe the facts and to draw our inferences from them, and not to attempt to describe the universe from our inner consciousness as we think it ought to be. All the results attained by science have been due to adherence to this method. Nevertheless it is not forbidden to entertain hypotheses before discovery, if such hypotheses are not valued for more than they

are worth. Another service which we imagine science renders to the community is the example which it offers of the reward of labor. The scientific man loves to work not only for the sake of acquisition, but also because of the pleasure there is in work as an activity of the human organism. By it we learn that by work only can great results be accomplished, and the law of conservation and correlation or energy teaches that something cannot be made out of nothing.

"In our educational function we hope by example to show that the mental life is as worth living and affords as much pleasure as the physical life. This is a lesson on which it is necessary to continually insist, since mankind is constantly prone to imagine that mental activity and thought are uninteresting and dull. On the contrary they afford a high class of pleasures which are conservative of the entire organism.

"We also emphasize the desirability of free thought on all subjects whatsoever, and the necessary corollary that the thought shall be careful and judicial. Thought so applied to our practical affairs must be in the highest degree beneficial in every direction both personal and national. We expressly repudiate two common types of thought. One of these attempts to prove by reasoning, if not by reason, a contention in which a person has an especial interest. It is to be feared that this habit of mind is too common, and it implies a lack of honesty of purpose which is entirely foreign to the scientific spirit. The other type of thinking to which we object is the acceptance of allegations concerning matters of fact or theory upon insufficient evidence, or upon authority only. Both of these methods lead to inaccurate results, and from both the scientific method protects us. I do not hesitate to say, that the future of science will be greater than its past, and that it affords a career to those who are adapted for it which promises a high degree of happiness and benefit. I believe that in this country with our facilities in various directions, the pursuit of science will become a more conspicuous part of our national life than it is now, and I am sure that nothing is more desirable for our national life than that this should be the case. In the cultivation of science we see the cultivation of honesty, of industry and of truth, all qualities which are essential to the prosperity of a people.

"Fellow citizens of Buffalo we thank you for the very material aid which you are rendering us in the attempt to develop this enterprise."

The Nominating Committee recommended the names of Prof. Wolcott Gibbs, of Newport, for President, for 1897–8, and of Dr. Asaph Hall, Jr., of Washington, D. C., for General Secretary; who were elected. The committee also recommended that a formal meeting for organiza-

tion only be held in 1897, at Toronto, in view of a cordial invitation from that place, and that it adjourn to assist the citizens in entertaining the British Association for the Advancement of Science, which is to meet there at that time. Other invitations were received from Nashville, Tenn.; Columbus, Ohio; Indianapolis, Ind.; Detroit, Mich.; Minneapolis, Minn.; Seattle, Wash., and San Francisco, Cal. The recommendation of the committee was not agreed to by the Association, who ordered that a regular meeting should be held, and referred the time and place to the council. At a subsequent meeting of that body it was agreed to meet in Detroit, commencing August 9th, in order to give the members the opportunity of attending the British Association meetting at Toronto thereafter.

Messrs. Tarr, Mayberry, Packard, Bessey and Carhart were appointed a committee to coöperate with the national educational societies in arranging the methods of science teaching.

Dringende Bitte

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Botanischen Jahresberichts

möglichst zu beschleunigen, wie eine Steigerung der Zuverlässigkeit in der Berichterstattung zu erlangen, richten wir an die

Botaniker aller Länder

die dringende Bitte um gefällige schleunige Zusendung ihrer Arbeiten, namentlich auch der Sonderabdrücke aus Zeitschriften, etc.

Alle Sendungen sind zu richten an den Herausgeber.

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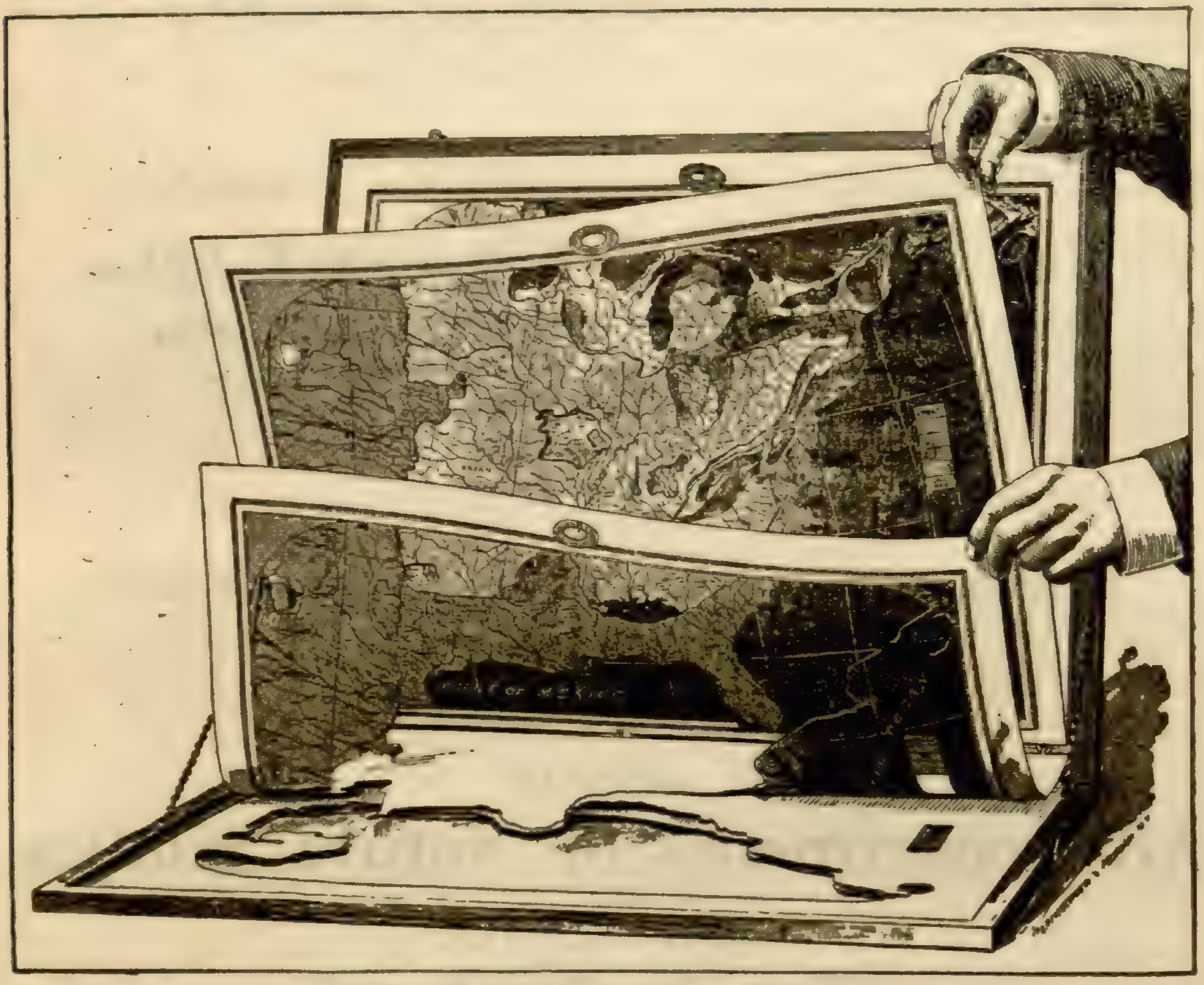

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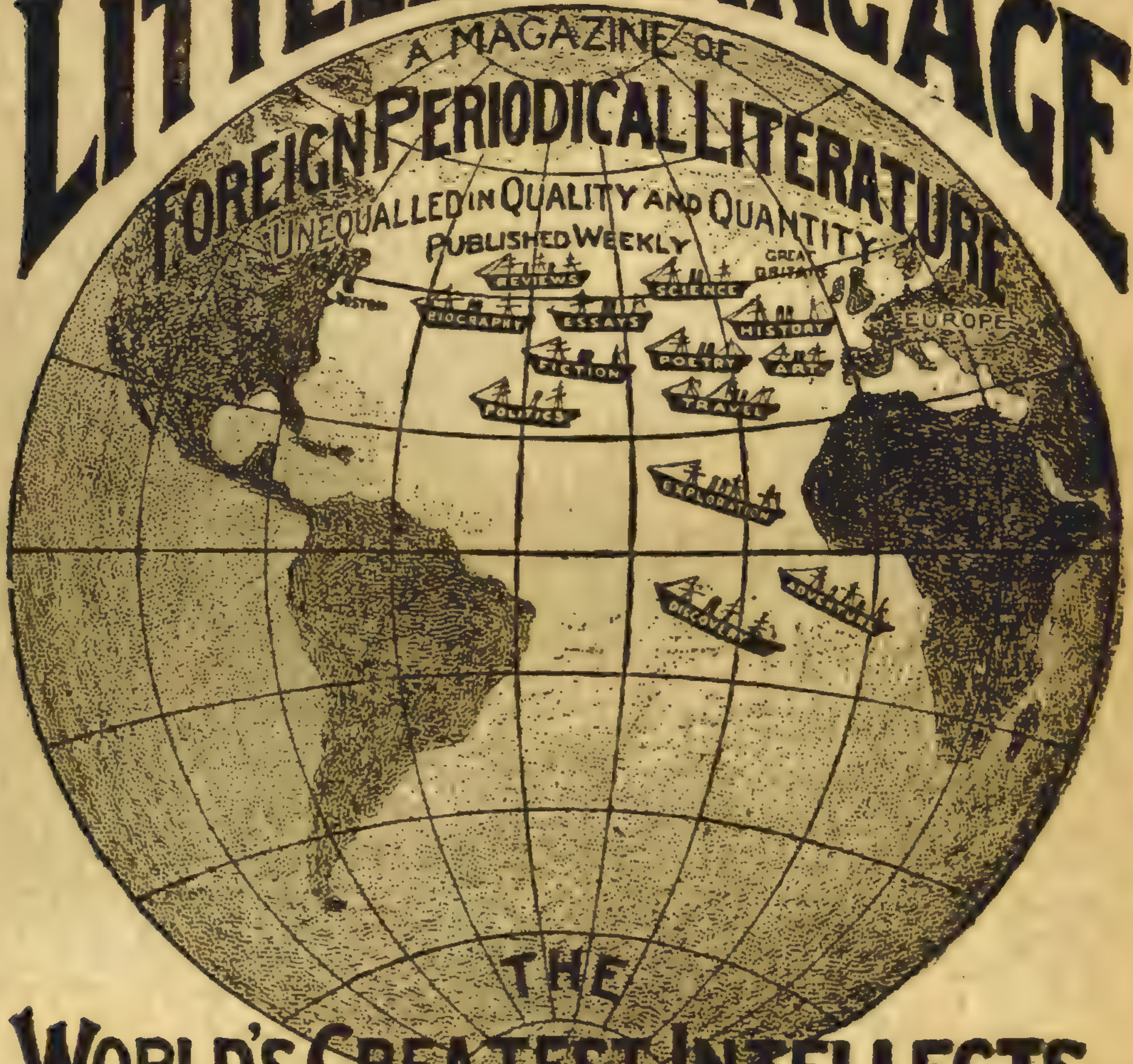

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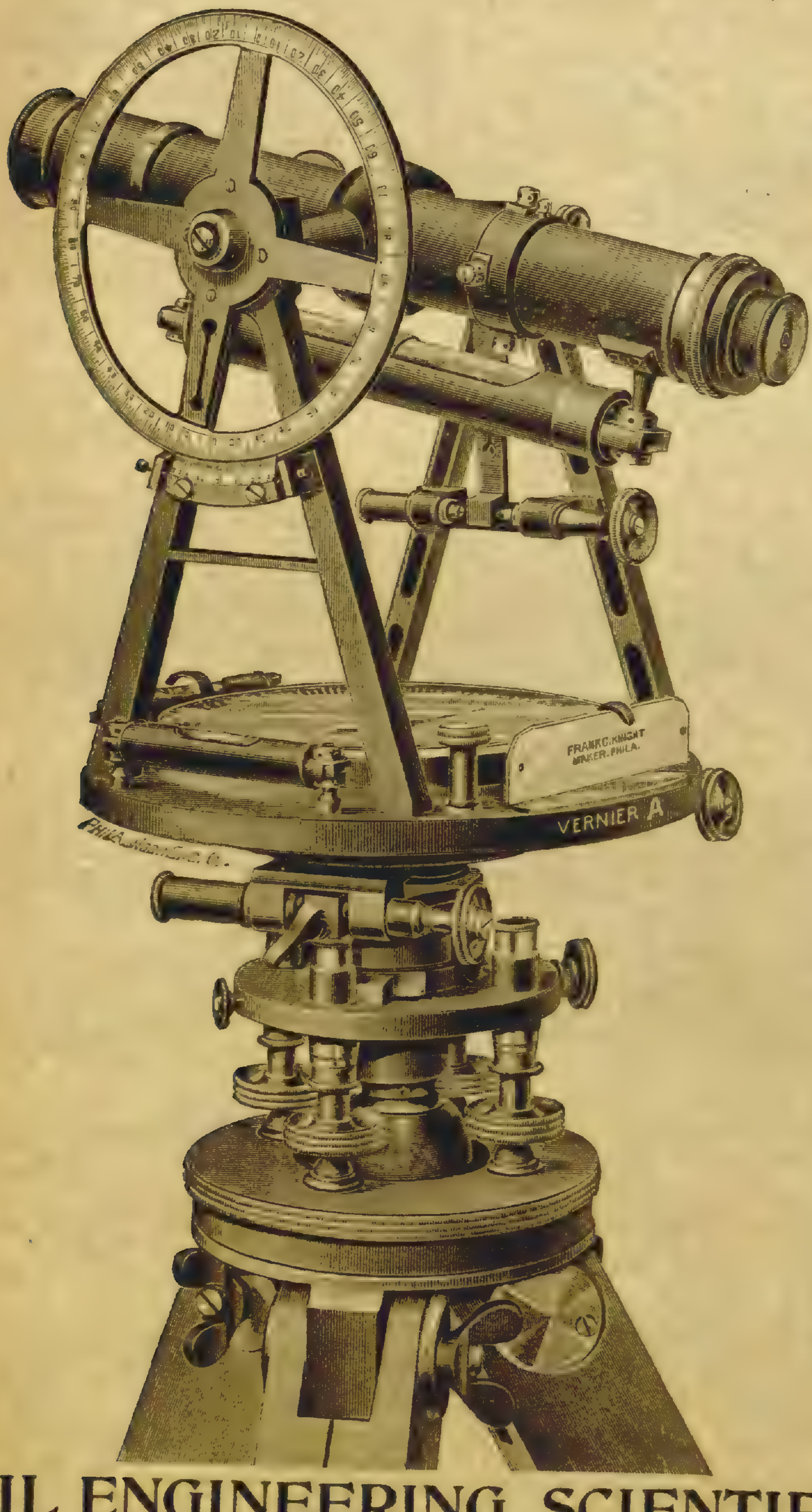

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THE
AMERICAN NATURALIST

VOL. XXX. October, 1896. 358

FRESH RELICS OF GLACIAL MAN AT THE BUFFALO MEETING OF THE A. A. A. S.

BY G. FREDERICK WRIGHT.

1. The first paper upon this subject was presented by Prof. G. Frederick Wright, detailing briefly the results of a single day's exploration at Trenton, N. J., under the guidance of Mr. Ernest Volk, who is continuously carrying on similar explorations under Prof. F. W. Putnam for the Peabody Museum at Cambridge and the Central Park Museum, New York City. Professor Wright was requested to select his own ground upon the Lalor farm, where permission has been given for exploration, and the surface to a depth of three feet would be dug over in his presence. The point selected is on the bluff of the terrace of glacial gravel upon which the city of Trenton is built, a mile or more south of the center of the city. The bluff here facing the river is about fifty feet above it, and the terrace stretches back in a dead level for a mile and a half. The situation is such that there was no chance for surface wash to have remodified the deposit. In the near vicinity were boulders two or three feet in diameter resting upon the surface, or slightly below it, showing the ordinary conditions of deposition in connection with floating ice which characterized the whole delta terrace at Trenton, and which have been so often described by the geologists who have visited the region.

A trench three feet deep and three feet wide was dug from the face of this bluff backwards about thirty or forty feet. The upper twelve inches of this trench consisted of sand discolored with vegetable decomposition, which had evidently been disturbed. In this stratum there were found two flint arrow-heads or spear-heads, one argillite chip, and one flint chip, together with a fractured pebble, four pieces of pottery, and a piece of charred bone.

The lower two feet of the excavation, except where interrupted by a pit, consisted of compact sand distinctly stratified, which had clearly been undisturbed. In this was found at varying depths one imperfect argillite implement, about three inches long and an inch and a half wide and a quarter of an inch thick, with five unrolled and angular fragments of argillite, two of which bore pretty clear evidence of having been chipped by human hands. These were the only fragments. There were no chippings or fragments of flint or jasper in the lower two feet of the excavation.

This brief paper of Professor Wright was but the prelude to bring out from Professor Putnam a fuller statement of the results of Mr. Volk's work on the Lalor farm. For two years Mr. Volk has been carrying on similar excavations over adjoining parts of the farm where the situation is similar to that described, and with corresponding results. Flint and jasper implements and flakes are abundant in the upper twelve inches of the soil, while no flint or jasper occurs in the lower two feet, of undisturbed sand and gravel. A large number of boxes of implements and fragments accumulated by this work of Mr. Volk have been sent up to the museums above mentioned; but, owing to the lack of time, Professor Putnam has not yet opened them and published the results. But in preparation for this meeting Professor Putnam had requested Mr. Volk to pursue further investigations and send the results to him at Buffalo. These were presented by Professor Putnam in a paper from Mr. Volk describing between thirty and forty argillite implements and fragments which had been found in his subsequent excavations in the undisturbed lower two feet of sand, as described in Professor Wright's excavation. As in that

case, so in this, flint and jasper were abundant in the upper twelve inches, but argillite was the only chipped and angular material found in the lower two feet. A large diagram accompanied Mr. Volk's description in which the position of each one of these argillite fragments was found. The box was then opened for the first time, and the fragments presented for examination. Of the artificial character of many of them there was not the least question on the part of any one present.

The importance of these discoveries as confirming the evidence of glacial man at Trenton heretofore presented can readily be perceived. It coincides with that presented by Professor Putnam and Dr. C. C. Abbott and Mr. Volk, going to show that there was a clearly marked succession in the human occupancy of the Delaware Valley indicated, first, by the sole use of argillite for implements, followed by a gradual and almost complete transition to the use of flint and jasper in later times. (See Putnam's report to the Peabody Museum in the Proceedings of the American Antiquarian Society, October, 1889, p. 11, and Observations upon the Use of Argillite by Prehistoric People in the Delaware Valley in Proceedings of the American Association for the Advancement of Science, by Ernest Volk, vol. xlii, p. 312). It also sweeps away at once the ingenious theories of Professor Chamberlin and others who would account for the occurrence of implements in the lower strata of sand and gravel through the agency of dry-weather cracks in the surface, the overturning of trees, the decay of tap roots, and the activity of burrowing animals; for none of these agencies would select the argillite, and leave the flint and jasper upon the surface. Therefore it would seem that there can be little doubt that these argillite fragments were scattered by the agency of man at the time that the deposition of the Trenton gravels was still in progress.

2. A second paper was by Prof. E. W. Claypole, detailing the particulars concerning the discovery of human relics from the drift at New London, Huron County, Ohio. These consisted of what would be called Neolithic axes, found by an intelligent workman in the process of well-digging in the blue till twenty feet below the surface. The circumstantial evidence

sustaining the testimony of the workman is of the most convincing character. The passage from the yellow till into the blue till and the occurrence of occasional strata of gravel are characteristic of the glacial deposits of northern Ohio. The implement had been subjected to oxydizing agencies characteristic of the deeply covered strata of that immediate vicinity. It is impossible briefly to detail this evidence. We must therefore wait for its full publication by Professor Claypole.

In a word, the geological situation at New London, Ohio, is this: The watershed between the Great Lakes and the Ohio is but a few miles to the south, and drains to the north through the main valley of Vermillion River. The land about New London is level for several miles, and is about two hundred feet below the summit of the watershed. There is no opportunity for any disturbances to have occurred subsequent to the glacial period; but in the retreat of the ice from the watershed a temporary glacial lake doubtless occupied the upper part of the valley of Vermillion River, emptying its waters into a tributary of the Mohican, and thence into the Muskingum and the Ohio. But this lake evidently did not exist for a great length of time.

Heretofore numerous flying reports of the discovery of implements in the glacial till have been made, but this is the first instance where the evidence has seemed in itself altogether convincing and satisfactory.

RELATIVE EFFICIENCY OF ANIMALS AS MACHINES.[1]

By Manly Miles, Lansing, Mich.

In my paper on Energy as a Factor in Rural Economy, read at the Washington meeting of the Association, approximate quantitative estimates were made of the energy expended

[1] Read in Section F. at the Buffalo meeting of the American Association of Science, Aug., 1896.

in the exhalation of water by plants, and evaporation from the soil; and at the Madison meeting similar estimates were given of the potential energy of an acre of corn, and of a fat ox, as representing the work done in the constructive processes of growth. The substance of the last mentioned paper, with some additional matter was published in the AMERICAN NATURALIST of July, 1894.

Some further illustrations of the same general principles are now presented in an inquiry as to the relative efficiency of different classes of animals, as machines, in utilizing the potential energy of their food in useful work.

From the imperfect data now available, there are many questions relating to this subject that cannot be definitely answered, but the approximate quantitative estimates we are able to make must be of interest in suggesting the lines of research required for a satisfactory solution of the problems involved in discussing the economy of foods and diets, and especially in the interpretation of the results of feeding experiments.

The chemical theories of nutrition have been so generally accepted in popular expositions of alimentary processes that it may be well to recapitulate the leading facts relating to energy as a factor in physiology in order to clear up the field of view and give due prominence to the principles we have to deal with.

The food consumed by animals serves two distinct purposes which should be clearly distinguished. The materials required in building tissues, and in the manufacture of animal products (meat, milk, wool, etc.), have alone been noticed in popular essays on the subject of nutrition, while the quite as important expenditures of the energy supplied in foods, as the motive power required in the constructive processes involved in converting the food constituents into animal tissues and products, have been misinterpreted or entirely ignored.

As pointed out in the papers above noticed, but a limited amount of the constituents of foods are stored up by animals in their processes of growth—in their increase when fattened—or in the animal products they manufacture. With refer-

ence to our present subject, the following table, showing the percentage of food constituents found in the increase of fattening animals at Rothamsted, will serve as a sufficient illustration.

TABLE 1.

Percentage of food constituents in the increase of fattening animals.

Constituents of Food.	Stored in Increase.		
	Oxen.	Sheep.	Pigs.
	Per cent.	Per cent.	Per cent.
Proteids	4.1	4.2	13.5
Carbo-hydrates and fat	7.2	9.4	18.5
Ash	1.9	3.1	7.3
Dry Substance	6.2	8.0	17.6

It will be seen that much the larger part of the food constituents were not utilized by the animals as materials for building tissues, but they have served a useful purpose in yielding up more or less of their stored energy, according to the degree of disintegration to which they were subjected, which was made available in the constructive processes of nutrition and the related incidental physiological activities of the system.

With this limited demand for the constituents of foods to serve as materials for tissue building, there must be an extensive disintegration of their organic substance to furnish the enormous supplies of energy required in the repair of tissues, in increase in growth, in the vaporization of water exhaled by the lungs and skin, and to supply the sensible residue that is lost by constant radiation from the body in the form of animal heat.

The obsolete theory of Liebig that certain food constituents are alone used to build tissues and that certain other constituents are burned in the system to produce heat, still continues to be the leading assumption in attempts to popularize chemical theories of nutrition in formulating diets and nutritive

ratios, and attention should be directed to the erroneous interpretations of organic processes that are made in the application of this false theory in connection with a fancied analogy of the animal machine to a steam engine.

We are told that "when coal is burned in the furnace a part of its potential energy is transformed into the mechanical power which the engine uses for its work. The rest is changed to heat which the engine does not utilize, and which, therefore, is wasted. The potential energy of the food is transformed in the body into *heat* and *mechanical power. The heat is used to keep the body warm. The mechanical power is employed for muscular work.*"

As an outcome of this false analogy the term "fuel value of foods" has been introduced to serve as an index of their capacity "to keep the body warm," and provide for muscular work. The absurdity of these crude and superficial views of energy as a factor in nutrition will be readily recognized by physiologists and we need only notice some obvious inaccuracies of statement.

In the first place, there are no processes of combustion in physiological activities, and fuel as such, can have no value in animal nutrition. The assumption that the potential energy of foods not expended in muscular work is "used to keep the body warm," is in direct conflict with familiar physiological activities. There are large expenditures of energy in transforming to the form of vapor the water exhaled by the lungs and thrown off as perspiration by the skin and it is a well known fact in physiology that the body is cooled by the evaporation of water from the surface that is constantly taking place.

The laboring man, perspiring freely in hot weather, expends a considerable part of the potential energy of his food in the cooling process of vaporizing the water discharged by the skin as a result of his exertions. The law of the conservation of energy is strictly observed and there is no demand for fuel to burn to "keep the body warm." The heat liberated in the destructive metabolism of the tissues, or what we speak of as the wear and tear of the sytem, is disposed of in various ways,

and the large expenditure in the cooling machinery for vaporizing water is an important factor in securing a proper adjustment in equilibrium of the numerous physiological activities of the system.

The fallacious and misleading theories of nutrition we have noticed have been so widely disseminated in recent official reports and bulletins that a detailed statement of the role of energy in animal nutrition, as now recognized by physiologists, seems to be required in relation to our present subject.

The energy required as the motive power in building tissues and the elaboration of animal products, as well as that expended in muscular work, is all derived primarily from the potential energy of the organic substances consumed as food, and as an incident of the various metabolic processes of the system animal heat is produced.

The stored, or potential, energy of organic substances is an essential element of their constitution, representing the work done in their processes of construction, and it can be liberated in the form of heat by any of the various methods of disintegration to which they may be subjected. For example, when organic substances are burned the heat produced is a measure of the energy stored up in their construction, but this method of liberating heat from food constituents has no place in physiological processes, and the value of foods as fuel is not a legitimate subject of discussion in domestic economy.

Microbes disintegrate the organic substances on which they feed and liberate their stored energy in the form of heat, as in the familiar processes of fermentation. The numerous microbes in the alimentary canal add their quota to the available energy in the form of heat derived from the constituents of foods which they tear apart in feeding upon them. The digestion of foods by animals is another means of liberating the stored energy in the form of heat.

Destructive metabolism, resulting from the various activities of the animal machine, immediately follows the constructive processes and the stored energy of the tissues originally derived from food constituents is liberated as heat which, so

far as needed, is used again as the motive power in rebuilding the disintegrated tissues and other physiological processes.

Animal heat is, therefore, the result of physiological activities that are carried on in accordance with the laws of the conservation of energy, without the slightest indication of anything analogous to processes of combustion.

Living substance, as pointed out by Foster, is matter constantly undergoing change, energy being used and stored up in constructive metabolism, and liberated again as heat in the correlated and quite as essential processes of destructive metabolism. Energy doing work, or stored as potential energy in the tissues, cannot be detected by the thermometer, and the heat liberated from foods in the various processes of disintegration they undergo, and from the tissues through destructive metabolism, is again made latent, as far as it is utilized in doing the work required in constructive metabolism, in vaporizing water exhaled by the lungs or thrown off by the skin as perspiration, and animal heat is the sensible residue not disposed of in these physiological processes.

Our domestic animals may then be looked upon as machines for doing work in the repairs and other vital activities of the animal machine itself, including muscular exercise, and the manufacture of animal products used as food by man. The importance of these animal machines as factors in domestic economy leads us to inquire as to their relative efficiency in utilizing the potential energy of foods in the special work they are fitted to perform.

Aside from the individual and class peculiarities that require attention, their efficiency in utilizing energy must vary with the quality and quantity of food consumed and a tentative solution of the problems presented in this line of inquiry can only be made. The quantitative estimates of the expenditures of energy in different ways we are able to make must, therefore, be interpreted as representing approximately the results with the particular animals under the conditions to which they were subjected.

The feeding experiments at Rothamsted, and the composition of different animals and their increase, as shown in the

extensive series of analyses made there, furnish the most convenient and only reliable data for our purpose in the case of fattening animals. The average results with oxen, sheep and pigs, as estimated by Sir John Lawes several years ago, and the feed consumed and increase made by the "analyzed fat pig" have been taken as the basis for calculating the expenditures of energy as given in the following tables.

In making a comparable estimate of the expenditures of energy in milk production, the record of a Guernsey cow at the New York experiment station in 1891, appeared to answer the required conditions, as detailed statements of the composition of the food consumed and milk produced were given, and the dry substance of the milk for each month differed but little from the dry substance of the 100 pounds of increase in the fattening animals under consideration, indicating in a general way that nearly the same amount of work had been required in its production.

The factors used in estimating the potential energy of organic substances (based on experiments of Berthelot and

TABLE 2.

Expenditures of energy in fattening oxen, sheep and pigs, to produce 100 pounds of increase in live weight; and a similar estimate of the expenditures of energy in milk production. In foot-tons of work.

	Potential energy of total feed consumed.	Expended and stored in 100 lbs. of increase.	Lost in excreta including urea.	In repairs of the system and other physiological processes.
Oxen	3,389,929	413,558	955,790	2,020,580
Sheep	2,809,787	440,584	655,967	1,713,240
Pigs	1,269,181	457,800	175,674	635,699
"Analysed fat pig"	1,478,393	435,160	272,335	770,900
		In 557 lbs. milk.		
Guernsey Cow — April	1,610,800	329,390	581,573	699,840
		In 581 lbs. milk.		
Guernsey Cow — May	1,626,678	349,440	564,020	713,230

Andre, 1890) represent in round numbers the calories per pound of the proximate principles of foods and tissues as follows: proteids, 2500 calories; fats, 4200 calories; and carbohydrates, 1900 calories.

For convenience of comparison, the energy in the table is represented in its equivalent of foot-tons of work; the energy expended in raising a weight of one ton one foot.

Without an examination of other details this table would be misleading in its indications of the relative efficiency of the different classes of animals as machines for doing work. There are decided differences in the quality and quantity of feed consumed which should be taken into consideration, but we have no data for a quantitative estimate of the modifying influence of these different conditions which can only be noticed in general terms. For convenience of comparison the most important facts relating to feed consumed may be summarized in tabular form.

TABLE 3.

Kind and quantity of feed consumed by fattening animals to produce 100 pounds of increase in live weight, and feed of the cow for equivalent results in milk production.

	Dry substance of feed.	Dry substance of increase.	Proteids in feed.	Proteids in increase.	Feed Consumed.		
					Hay.	Grain.	Green feed.
	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Oxen	1109	68.6	218	9	600 (clover)	250 (oil cake)	3500 (swedes)
Sheep	912	72.5	177	7.5	300 (clover)	250 (oil cake)	4000 (swedes)
Pigs	420	73.8	52	7.0		500 (Barley meal.)	
"Analyzed fat pig"	478	71.4	100	7.7		1. Bran. 2. Bean and lentil meal. 3, Barley meal.	
Guernsey Cow April	469	78	82	19.2	300 (clover and	210 mixed	420 corn
Guernsey Cow May	510	83	81	21.5	182 timothy)	213 grain	868 ensilage

To produce 100 pounds increase the oxen consumed more dry substance of food than the sheep, and of coarse fodder there was twice as much hay requiring a larger expenditure of energy to dispose of it, and a smaller amount was utilized and stored in the increase which contained less dry substance. There was also a larger loss in the excreta and a greater expenditure in repairs of the system and other physiological processes, resulting from the increased metabolism.

A comparison of tables 2 and 3 will show that less work is required to dispose of the smaller amount of coarse feed in the rations of the sheep, and with less dry substance of feed they give a much better proportionate return in increase than the oxen; while the pigs fed on barley meal alone make the same increase from less than one-half the dry substance of feed consumed by the sheep, with a diminished waste in excreta, and the repairs of the system and other physiological processes are carried on with a comparatively small expenditure of energy.

The fourth column of table 3 is, however, of special interest in relation to the processes of nutrition, and the difference in the results we have been discussing. One of the most marked results of proteid substances in the food, now recognized by physiologists, is to increase the metabolism of the system. From the small amount of proteid substance stored up in the increase of fattening animals, as shown in tables 1 and 3, an active metabolism of the system must be carried on to dispose of any considerable excess supplied in the food, as all that is digested and not retained in the increase is discharged by the kidneys in the form urea. The supply of energy to carry on the increased metabolism of the system arising from an excess of proteid food is, to some extent, however, immediately provided for in the heat liberated in its conversion into urea.

The oxen, with 218 pounds of proteids in their feed, give the smallest return in increase, the largest amount of waste in excreta, and decidedly more work is required in repairs of the system and other physiological processes resulting from the increased metabolism. The pigs fed on barley meal, with only

one-fourth as much proteid matter in their feed, have a decided advantage in utilizing energy in their increase, with less waste in excreta, and a comparatively small expenditure of energy is required to keep the machine in working order.

A comparison of the results with the pigs fed on barley meal and the "analyzed fat pig" with a highly nitrogenous diet, will furnish quite as striking an illustration of the influence of proteid food, as the conditions are less complex from the absence of coarse fodder in the rations.

The "analyzed fat pig" consumed more dry substance, and nearly twice as much proteid matter, to make 100 pounds increase in live weight as the pigs fed on barley meal alone, but less energy was utilized in its increase, containing less dry substance and decidedly more was lost in excreta and a larger amount was required in repairs of the system.

TABLE 4.

The energy of food consumed by different animals to produce a given increase is expended as follows, under conditions above noticed.

	In increase.	Lost in excreta.	In repairs and other physiological processes.
	Per cent.	Per cent.	Per cent.
Oxen	12	28	60
Sheep	16	23	61
Pigs	36	14	50
"Analyzed fat pig"	29	18	52
Guernsey Cow, April	20	36	43
Guernsey Cow, May	21	35	44

The pigs fed on barley meal with their comparatively simple organs of nutrition, require less dry substance of feed and less energy to make a given increase than the ruminants with their complex nutritive machinery and large amount of coarse fodder in their rations which they are fitted to utilize; but pigs and ruminants are alike in failing to give as large returns in profitable increase with an excess of proteids in their feed.

The results recorded in table 2 must then be attributed to differences in the working machinery of the animals themelves, and the modifying conditions of the relative amount of coarse fodder and proteids in the feed consumed, but with our present knowledge of vital activities the relative influence of these variable factors cannot be determined.

The expenditures of energy by different animals in different ways, given in table 2 in foot tons of work, are summarized in percentages of the energy of feed consumed in the following table for convenience of comparison.

In the last columns of tables 2 and 4, several well defined physiological processes are grouped together from the lack of data to discriminate between them, and they undoubtedly vary in the relative expenditures of energy required in them under different conditions. Without attempting an exhaustive enumeration of these processes, the following may be noticed as of the first importance from the work performed in their activities.

Constructive metabolism in the repair of tissues; vaporization of water exhaled by the lungs and skin; work of the involuntary muscles in respiration and circulation of the blood; energy expended in mental activities and the functions of secretion and excretion; loss of heat by radiation from the body; and work done by the voluntary muscles.

It will be seen from table 4 that the largest percentage of the available energy of foods is expended in these strictly physiological processes concerned in maintaining the integrity of the animal machine. Under the conditions of feeding experiments, with fattening animals and cows giving milk, but little mechanical work is done by the voluntary muscles, and the last item of our enumeration of physiological processes might have been omitted as insignificant in relation to the enormous expenditures of energy in the other normal activities of the system to which attention has been directed.

If mechanical work is done by animals, it must be at the expense of the energy that might, under other conditions, be expended in the manufacture of animal products, as the physiological processes enumerated above must all be provided for

in fattening animals and cows giving milk with a minimum of muscular exercise, as well as in the case of animals engaged in severe muscular work.

The energy expended in mental activities is of the first importance in its influence on the efficiency of the animal machine in useful work. The nervous system, through which mental endowments are manifest, has intimate relations with every part of the animal machine and the direction in which energy is expended is largely determined through its agency.

Practical farmers are well aware that animals fail to give profitable returns for feed consumed when restless and excited through any source of disturbance, or when dissatisfied with their feed and surroundings.

The available energy of a liberal supply of nutritious food may all be expended, and even the stored energy of the tissues drawn upon to carry on the increased physiological activities resulting from mental and nervous derangements of the nutritive machinery without any expenditure in profitable production. In conducting feeding experiments and in the interpretation of their results this is one of the most difficult factors to deal with, as it may have a dominant influence on the final outcome.

The approximate estimates of the relative efficiency of different animals in utilizing the potential energy of their feed in useful work, which have been given in mere outline, will require revision and correction as we become better acquainted with the specific influence of the variable factors of food and environment on the work performed by animal machines. Even in their present imperfect form they may, however, serve to illustrate the significance of energy as a factor in animal nutrition and the futility of formulating diets and nutritive ratios in terms of their chemical constituents.

THE BACTERIAL DISEASES OF PLANTS: A CRITICAL REVIEW OF THE PRESENT STATE OF OUR KNOWLEDGE.

By Erwin F. Smith.

(*Continued from p. 731*)

III.

Note.—A second note by Dr. Sorauer on the Bacteriosis of Fodder Beets adds a number of interesting items. This is entitled (***24a***) Die Gummikrankheit bei Runkelruben etc. It was published in *Jahrb. d. Deutschen Landwirtschafts-Gesellschaft.* Bd. 7, Berlin, 1892, Second part, pp. 206, 207, and was republished verbatim in *Zeitschr. f. Pflanzenkr.* 2Bd. 5Heft, 1892, pp. 280–281. The following abstract should therefore be read in connection with the *Remark* on p. 723 from which place the reference was inadvertently omitted.

Samples of the diseased beets were received from Vukovar in October, 1890 and in February, 1891. With the first specimens came the statement that one half of the crop was affected. The beets were drilled early in April and the weather was favorable until the middle of June; then hot and dry weather set in, continuing until harvest time in the middle of October. During all this time there was only one rain. The beets suffered severely from the heat and drouth, losing all their outer leaves and pushing new ones toward the end of September. At harvest time the disease was found more or less developed in such plants as showed wilted heart leaves. The blackening of the roots, a blue black, began at the lower extremity and continued upward after the roots were stored. The flesh of the root appeared to be uniformly blackened at the root tip, and from this part the discoloration radiated upward into the sound part so that at last it was noticeable only in the regions of the vascular bundles as brown stripes and rings, the rest of the flesh being white. On making a cross section of the beet root a drop of gum sometimes exuded within a few minutes from isolated points of the browned vascular strand. The affected beets became flabby and shriveled lengthwise and in places showed a sticky sweat or an abundant exudation of gummy masses through the uninjured surface, forming a lacquer-like covering. In bad cases gum cavities appeared in the flesh from solution of the tissues. The diseased beets contained a strikingly large amount

of grape sugar. This was determined by use of Barfold's reagent. Two milch cows fed on these roots died the second day. The symptoms were bloating, pain in the abdomen, obstinate constipation, and the prolonged vomiting of a tough yellow slime. In both cases pieces of the diseased beets were found in the first stomach along with other food, and no other reason for death was apparent. These beets (four varieties) were grown on a porous clay soil containing 5% lime and 8% humus. The soil-water level was 16 meters from the surface. The field had been very heavily dunged with stable manure.

II. THE HYACINTH (HYACINTHUS ORIENTALIS L.).

1. THE YELLOW DISEASE (1883).

(I) THE DISEASE.

(1) *Author, Title of Paper, Place of Publication.*—This disease was described by Dr. J. H. Wakker, as a result of investigations begun in the fall of 1881 at the request of the Algemeene Vereeniging voor Bloembollencultur te Haarlem, and was carried on in the laboratories of the University of Amsterdam. The first brief account appeared in 1883, in *Botanisches Centralblatt* (Bd. XIV, pp. 315–316) and forms part of a (**30**) Vorläufige Mittheilungen über Hyacinthenkrankheiten. The following year a more extended account was published in Dutch, (**31**) Het geel-of nieuwziek der Hyacinthen veroorzaakt door *Bacterium Hyacinthi* Wakker (*Onderzoek der Ziekten van Hyacinthen, en andere bol-en knolgewassen. Verslag over het jaar 1883.* Haarlem, August, 1884. 8vo, pp. 4–13, one colored plate). Two additional papers were published in Dutch, continuing and concluding the one above mentioned, (**32**) *Onderzoek etc., Verslag over het jaar 1884.* Haarlem, May, 1885, 8vo, pp. 1–11, and (**33**) *Onderzoek etc., Verslag over het jaar 1885.* Haarlem, May 1887, 8vo, pp. 1–5, and 27 to 37). These are the important papers to read and the ones which have mostly escaped mention. Finally, six years after the commencement of the investigation, Dr. Wakker published a fifth paper, entitled (**34**) Contributions à la pathologie végétale: I. La maladie du jaune, ou maladie nouvelle des jacinthes, causée par le *Bacterium Hyacinthi* (*Archives néerlandaises d. Sci. ex. et nat.*, tome XXIII, 1889, pp. 1–25, pl. I). This paper is merely

an abstract of the earlier Dutch papers. Part of the lithographic figures in the plate are, however, new.

Remark.—The papers published by Dr. Wakker in 1883 and 1884 were among the first contributions of any importance to the bacterial literature of plant diseases, but they were not, as claimed, actually the first. That honor belongs to this country, as we shall see later on when we come to take up pear blight, Prof. T. J. Burrill having published a long paper in 1880. The lack of literature and the difficulties in the way of the successful prosecution of this work at that time are well expressed in the *Verslag* for 1883: Als oorzaak van een plantenziekte waren Bacteriën nog slechts eenmaal en dot wel zeer kort (Prillieux *Bull. d. l. soc. bot. d. Fr.* 1879, p. 31 and 187) beschreven zoodat het geval van het geelziek niet alleen van praktisch, maar ook van hoog wetenschappelijk gewicht is. Is daarom het onderzoek er van zeker her belangwekkendste van alle ziekten, waarmede wij ons hier zullen bezighouden, het is ook tevens het moeilijkste omdat bij gebrek aan mededeelingen omtrent dit of een dergelijk onderwerp alles op eigen onderzoek berusten moet.

(2) *Geographical Distribution.*—This disease has prevailed extensively at times in the large bulb gardens in the Netherlands, where it is said by a majority of the Dutch horticulturists to be a new trouble, i. e., one that has appeared within the last ten years (**31**) or within the last 20 years (**34**). The writer of this digest has never been able to find the disease in bulbs imported from Holland, and does not remember to have seen any account of its occurrence in other parts of the world.

(3) *Symptoms.*—According to Dr. Wakker the first symptoms of the disease are usually in the foliar and floral organs. There is an apical browning or blackening of the leaves and scapes which color can often be traced downward into the green leaves for some distance in the form of dark stripes. The epidermis frequently ruptures longitudinally, and large irregular masses of bacterial slime exude from the rifts. The diseased parts also have a wet, unctuous appearance, and shrivel from the apex downward. Subsequently the bulbs become diseased, and clearly as a result of the preceding disease of the

foliar and floral organs. The earliest symptom of disease in the bulb consists on cross-section, of yellow dots, visible here and there in the interior of the scales or, on longitudinal section, of yellow lines which frequently extend into the plateau. From these spots, a mucilage swarming with bacteria can be obtained in drops by squeezing the scales or simply by exposing the cut bulb to the air. Sometimes the whole interior of the scale, or all of its inner or outer part, degenerates into a mass of yellow slime. If the attack is rapid, the plateau is soon invaded and the bulb rots in the ground during the fall or winter. If the progress of the disease is slow, and this is usually the case, the bulb sends up leaves and blossoms the following spring in the ordinary manner. At this time or afterwards there are distinct signs of the disease. In many cases the leaves turn yellow in lines parallel to the longer axis of the leaf. These lines begin at the base of the leaf and proceeding upward become less and less visible until they entirely disappear. In the interior of the leaf under these yellow stripes the bacterial slime is abundant and on the lower parts of the leaf it frequently finds its way to the surface, ruptures the epidermis and escapes. In such plants the bulb is always badly diseased, especially the outer scales which are the basal portion of the leaves of the previous year, and this, taken in connection with the fact that the bulbs are entirely sound in most cases where the symptoms are only visible at the apex of the leaf, renders it very probable that the latter is to be regarded as the first stage of the disease and the former as a later stage, supervening the second year. Another symptom, sometimes observed the second year, is unequal growth, i. e., a distinct curving over of the foliage toward that side of the plateau which has perished or is no longer capable of furnishing the proper supply of water and nutrient substances, the curvature being, of course, due to the one-sided growth. As a rule, diseased bulbs do not produce many bulblets, and not all of the latter are always diseased. If planted out, those which are diseased show signs of the malady in the young plants after a longer or shorter period. The leaves turn yellow, become flabby and droop, or show the characteristic longitudinal rifts in the epidermis. When such

young bulbs are cut, the plateau (central short stem of the bulb) is often found to be the only diseased part, something inexplicable if we do not admit that the disease has been transmitted to the bulblet from the mother bulb. Such is the usual form of the disease, but it will be understood that there are numerons modifying circumstances, the disease sometimes beginning lower down on the edge of the leaf, or even underground, or progressing more rapidly, the latter especially when the disease attacks full grown leaves and scapes. The most cases of the first stage of the disease are noticed in the field in May, but cases also occur much earlier in the year.

(4) *Pathological Histology.*—In spring, in the first stage of the disease, when only the tips of the leaves are attacked, microscopic examination shows the bacterial slime to be present in the intercellular spaces of the shrivelling leaf-parenchyma, but always only in small quantity. From this part of the leaf the bacteria may be traced long distances down the vascular bundles, but have not yet reached the bulbs, the latter being still entirely sound. In autumn, on the contrary, cross sections of the bulbs, if not too badly diseased, show numerous yellow dots in the scales, and on microscopic examination these are found to correspond to the xylem part of the vascular bundles (No. **34**, pl. I, figs. 9, 10). The vessels of the latter are seen to be full of a thick, yellow slime, which often partially dissolves them. Here and there, the whole xylem part of the bundle may disappear, the yellow slime taking its place. In this way are formed continuous, tubular cavities, filled with isolated cells of the host plant, remnants of spiral threads, and an innumerable number of bacteria. In this stage of the disease, the sieve tubes are not yet attacked, but these are subsequently destroyed, and frequently, also, the parenchymatic tissues outside of the bundles, the substance which unites the cells being first dissolved. The second spring, a microscopic examination of the yellow striped leaves from diseased bulbs shows a similar occupation of the vessels with the same lesions, but in a reverse order, the bacteria being most abundant and the destruction of tissues greatest in the basal part of the leaf. Here the bacteria dissolve the walls of the vessels

and make their way into the surrounding tissues, first isolating and then destroying the parenchymatic cells and finally increasing to such an extent that the epidermis is ruptured and a viscid, yellow ooze escapes. The vessels of the scape are filled in the same way, but the bacterial slime was not observed in the roots. The bacteria may be distinguished in the vessels considerable distances in advance of any external or macroscopic symptoms. The bacterial mucilage is a yellow liquid, thick and viscid. Under a low power of the microscope it shows a granular structure, this being due, as we recognize on higher magnification, to the presence of bacteria. Dr. Wakker describes at some length his method of examination: "The transverse sections must be made with extreme care, such as is almost unknown in the ordinary study of vegetable anatomy. Not only is the affected tissue so soft that it is impossible, in a fresh state, to cut sections sufficiently thin, but there is also danger that the mucilage will be dragged by the knife into parts of the leaf where originally there was not a trace of it. To overcome these two difficulties good use was made of absolute alcohol. The green color was removed by this method while the yellow of the mucilage persisted, and, in consequence, became much more distinct, so that it was easier to distinguish diseased from healthy tissues."

(5) *Direct Infection Experiments.*—This disease was studied before Koch's plate method of isolation had come into general use, and most of the infection experiments were made directly from diseased to healthy plants. In the fall of 1882 bacteria were introduced into a bulb of the double white Anna Maria, and when this bulb was cut the next spring it showed distinct signs of the disease. This experiment was frequently repeated and always with the same result. For example, the whole cut surface of the scales of a bulb from which roots and leaves were cut away in summer was smeared with the bacterial slime, and in 14 days the disease was to be found in the vascular bundles of the youngest scales, and shortly after in those of the older. Slightly diseased bulbs are the best parts from which to obtain the bacteria. In badly diseased plants one runs the danger of finding all sorts of things, even Penicillium, in the de-

cayed mass; and from the leaves, before the disease has reached the bulb, it is impossible to get a sufficient quantity of the slime.

On March 27, 1884, small quantities of yellow slime were taken from some slightly affected bulbs (double red Temple of Apollo) and inserted into wounds made for this purpose in the top of the leaves of different varieties. These were examined daily for signs of disease which first appeared, in most cases, only after a month. Distinct symptoms were apparent but unfavorable circumstances caused the loss of the leaves before the downward stripe had progressed very far. These plants stood in pots in the open air and were watered regularly, but the spring was very dry. Although in this case a month elapsed before external symptoms appeared, it is not to be inferred that so long a time always intervenes. On the other hand it is likely that in natural infections even a longer time may elapse before symptoms appear, since countless numbers of the bacteria are used in artificial infections, while natural infections are probably brought about in most cases by the entrance of a few bacteria which would require more time to produce visible results. On Oct. 27, 1884, the small unfolding leaves of each of a number of sound hyacinth bulbs were wounded with a steel pen and some of the bacterial slime inserted into these punctures. The bulbs were then potted, kept in a place free from frost and examined from time to time. On Jan. 13, 1885, one plant showed the disease very distinctly. Two of the three infected leaves had stripes extending downward from the wound, each about 13 mm. Here, also, a long time intervened between the inoculation and the appearance of external symptoms.

Another series of infection experiments was begun Dec. 28, 1885, and completed in the spring of 1886. These plants were also inoculated with bacterial slime taken directly from diseased plants. These experiments were made on five plants of as many varieties, grown in carafes. All were kept in a cool place until Feb. 13, when they were transferred to a room regularly warmed. The manner of inoculation and the results obtained are here summarized: *La Tour d'Auvergne* (double

white variety). Bacteria introduced into a wound in the yellow part of a leaf. *Results:* Feb. 13. In bloom. Two small streaks extending downward from the wound. Feb. 20. Leaf accidentally broken. It was put immediately after into alcohol and subsequently hardened in absolute alcohol, whereupon microscopic preparations made and stained in the way already described, showed three vascular bundles attacked in varying degrees. Two of them evidently corresponded to the two little stripes on the leaf, while in the third bundle the disease had not made enough progress to be visible on the surface of the green leaf. *Norma* (single red variety). Bacteria introduced into the green tips of three leaves. *Results:* Feb. 13. In bloom. 1st leaf, nothing. 2nd leaf, a little spot two millimeters above the wound. 3rd leaf, a similar spot below the wound. Feb. 20. No change. Feb. 27. No change. March 6. No change. March 20. Second leaf broken; 3rd leaf show little spots 10 mm. below the wound, plant moved into the open air. March 27. No change. April 3. No change. *Coeur blanc* (single white variety). Bacteria introduced into a wound in the yellow part of a leaf. *Results:* Feb. 13. A stripe extending downward from the wound, 4 mm. Feb. 20. In bloom. No change. Feb. 27. Length of stripe 15 mm. March 6. Length of stripe 22 mm. March 13. Length of stripe 25 mm., and small spots 10 mm. lower. March 20. Length of stripe 27 mm., and small spots 22 mm. lower. Plant put into the open air. March 27. Length of stripe 27 mm., and small spots 35 mm. lower. April 3. Length of stripe 67 mm. *Crown Prince Charles of Sweden* (double blue variety). Bacteria put into wounds at the green apex of two leaves. *Results:* Feb. 13. Nothing. Feb. 20. 1st leaf, a downward stripe of $3\frac{1}{2}$ mm. from one of the wounds. 2nd leaf, nothing. Feb. 27. 1st leaf, length of stripe 15 mm., and small spots all around the wound. 2nd leaf, nothing. March 6. In bloom. Length of stripe 17 mm. March 13. Length of stripe 18 mm., and a small spot 4 mm. lower. March 20. Very little change. Plant put under a bell jar on a dish containing water. March 27. Length of stripe 22 mm. Bell jar removed because leaf began to turn

yellow. April 3. No change. *Anna Maria* (double white variety). Fragments of diseased tissue introduced into wounds in the green tips of three leaves. *Results:* Feb. 13. A downward stripe from one of the wounds, length 10 mm. On the other two leaves nothing. Feb. 20. In bloom. Length of stripe, 17 mm. On the other two leaves nothing. March 6. Length of stripe 35 mm., and small spots 10 mm. lower. March 13. Length of stripe 45 mm. The stripe and bordering tissues have dried up for a distance of 35 mm. March 20. Length of stripe 55 mm., and small spots 15 mm. lower. Diseased part dry for a length of 50 mm. Leaf bent by the drying of one side. Plant put out-doors. March 27. Length of stripe 90 mm. Dry for a length of 55 mm. April 3. Length of stripe 94 mm.

Measurements were not made after April 3, but subsequently all of the diseased leaves were removed, placed in alcohol, hardened in absolute alcohol, and examined microscopically in the same manner as the leaf already mentioned, and with the same result. On the same date, Dec. 28, 1885, a quantity of bulbs, including the above varieties, were also infected and were planted out of doors where they were exposed freely to the air. Up to April 3 there were no signs of disease but a little later symptoms appeared in most of the plants. From these experiments the author draws the following conclusions: (1) the *Geelziek* or *maladie du jaune* can be induced artificially, and (2) the results of the infection make their appearance a long time after the operation.

(To be continued.)

EDITOR'S TABLE.

THE late meeting of the American Association for the Advancement of Science while less numerously attended than some others, was a larger gathering than has sometimes represented it. The meetings of the Association cannot be as large relatively to our population as those of most of the European nations, because of the longer distances which the members are compelled to transverse in order to reach them. Many of the most active workers must always be absent in the field during the summer months, especially so long as our country presents such opportunities for original research. The summer schools take away some members. The meeting at Buffalo was held in such a way as to discourage the attendance of those who regard it as merely an opportunity for junketting. The meetings extended from Monday to Friday inclusive, and Saturday only was reserved for excursions. This arrangement was greatly to the advantage of work, the maintainance of interest, and of the attendance. The members present were more than usually conspicuous as workers, and the number and value of the papers read was fully up to the best standard.

The Association decided to meet in Detroit at the unusually early date of August 9th, next year. This date was fixed on account of the approaching meeting of the British Association at Toronto on August 18th following. A cordial invitation from the citizens of Toronto to take part in the reception of the British Association was accepted, and this will follow the meeting at Detroit.[1] A respectable minority of the Association thought that we should suspend our meeting for that year, or meet formally for organization only, and then adjourn to take part in the reception of the British Association. This view carried the Nominating Committee, but was not approved by the Association. That the Association did wisely there can be no doubt, and the circumstance shows that all the wisdom in that body is not concentrated in its representatives in the Nominating Committee. The reasons put forth by the Committee for its action were plausible, but were believed to be fallacious by a large majority of the Association. One of these reasons was the assumption that the American Association meeting would necessarily be neglected by its members if the British Association meet in Toronto. The Association thought otherwise, especially as it was remembered that the second largest meeting ever held was in

[1] Not however by special adjournment as stated in Nature of Sept. 17, p. 480.

Philadelphia in 1884 when the British Association met in Montreal. As the American Association knows its own mind, we may look for one of our largest meetings in Detroit in 1897.

In our issue for October, 1895, we referred to the organization of the Field Museum of Chicago as having failed to furnish a successful basis of operations for the prosecution of original research. At that time most of the men who could give reputation to it had left, owing to the unsatisfactory positions in which they found themselves placed. Subsequently the establishment of publications of a very meritorious character induced us to believe that proper steps had been taken by the management to place the scientific men on such a basis as to insure the future prosperity of the enterprise. Authentic information recently received shows that this anticipation was premature. Other resignations have occurred, and the institution is evidently destined to be a failure unless a reorganization is effected.

Men who have spent their lives in mercantile pursuits are generally unacquainted with the conditions necessary to original research in science. The modus operandi in the two pursuits is fundamentally different. An element of tentative experiment enters into the pursuit of science, which requires a degree of freedom on the part of the investigator which cannot be accorded to the regular employee, the results of whose work are always susceptible of full anticipation. The investigator must have full control of material of research and of the ways of getting it. In fact no one else is likely to know how to get it. He alone knows the profitable lines of work; hence he must be permitted to select his work. No one will secure a museum sooner than he, and it will be as much more valuable than can be created by any one else, as the work of an expert is necessarily more important than that of other persons. For these and many other reasons no museum can become great unless its administration is in control of scientific experts, who should be responsible to each other and to the trustees only. With an organization of this kind, composed of the class of men from whom it has already selected some of its aids, there is no reason why the Field Museum, under the liberal terms of its endowment, should not rival the greatest museums of the world.

—We must again remind contributors to the Naturalist that proofs of all kinds and blocks of engravings must be sent to the publishers and not to the managing editor. Failure to observe this rule often causes inconvenient delays. Manuscripts, on the other hand, should go to the appropriate editors, and not to the publishers.

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General Notes.

MINERALOGY AND CRYSTALLOGRAPHY.[1]

Development of Faces on Crystals.—Gaubert[2] makes a contribution to the subject of the growth of crystal faces by means of his experiments with the alums. An octahedron of chrome alum, on solution in its mother liquor, is rounded at its edges and angles. When the solution becomes again saturated, and the crystal begins to grow, faces of the forms (100), (110), (211) and (221) are developed, but disappear on continued growth, leaving finally only the octahedron (111). Experiments with crystals of chrome and potassium alum prove that the same faces are developed when the rounding is done mechanically instead of by solution. Potassium alum from pure water gives the form of octahedron and cube, but by rounding (211) and (221) may be caused to grow.

[1] Edited by Prof. A. C. Gill, Cornell University, Ithaca, N. Y.

[2] Bull. Soc. Fr. Min., XVIII, pp. 141–143, 1895.

Crystals of lead nitrate and of barium nitrate also develop transitory faces when rounded, returning to the original form of cubo-octahedron on continued growth. Miers has observed the formation of the face (221) by the extremely slow solution of the potassium alum crystals. Hence, it seems that these "transitory planes" may be formed either by corrosion or by growth of a rounded crystal.

Albite from Lakous, Island of Crete.—Viola,[3] by his paper on the new occurrence of albite at Lakous, adds another to the list of carefully investigated pure chemical substances. An analysis by Mattirolo, given at the end of the article, shows close agreement with the theoretical values for Na Al Si_3O_8 as may be seen from the following:

	Found.	Theoretical.
SiO_2	68.35	68.70
Al_2O_3	19.78	19.47
Na_2O	11.71	11.83
K_2O	.16	
Ign	.15	
	100.39	100.00

Measurements on twelve crystals, varying from $7\frac{1}{2}$ to 20 mm. in diameter, agree very well in giving as crystallographic constants: $\alpha = 94° 14' 30''$, $\beta = 116° 31' 45''$, $\gamma = 88° 5' 1''$, and $\breve{a} : \bar{b} : \dot{c} = .635 : 1 : .557$. The extinction angle measured against the trace 001 in a section cut parallel to 010 is 21° 30′, in the section 001 it is 3° 30′. The optical angle is approximately + 80°. Inclusions of a member of the chlorite group are found in a number of the crystals, and some small scales of hematite in others.

Forsterite from Monte Somma.—The specimens seem to be of unusual chemical purity, hence the data given by Arzruni[4] on the the basis of investigations by himself, Jolles and Thaddéeff are doubtless near the true values for pure $Mg_2 SiO_4$. The axial ratio is found to be a : b : c = .46663 : 1 : .58677. Cleavage parallel to 010, distinct. In addition to the previously observed method of twinning, the plane 031 is reported as a twinning plane.

[3] Tscherm. Mitth., XV, pp. 135–158, 1895.
[4] Zeitschr. f. Kryst., XXV, pp. 471–476.

The plane of the optical axes is the base, and the optical angle is 85° 38′ for lithium, 85° 45′ for sodium, and 85° 56′ for the thallium light.

The results of analysis are:

	I.	II.
SiO_2	42.65	42.39
FeO	1.35	3.12
MgO	56.57	55.09
CaO	.29	
Al_2O_3		.23
	100.86	100.83
Sp. G.	3.223	3.245

The ratios $RO : SiO_2$ are 2.018 : 1 and 2.01 : 1 respectively, after deduction for probable impurities.

Fayalite and the Chrysolite-Fayalite Group.—Penfield and Forbes[5] found the fayalite from Rockport, Mass., suitable for optical and other investigations. The mineral was found in the shape of a lenticular shell in massive hornblende-biotite granite. The color is a dark resinous green, though the light transmitted by the thin edges is yellowish. The purified powder has a specific gravity of 4.318 (average of 3 determinations). The average of the two analyses is:

SiO_2	30.08
FeO	68.12
MnO	.72
H_2O	.80
	99.80

The cleavages are 001 and 010, and the reported occurrence of a cleavage 100 is considered a mistake.

The plane of the optical axes is the base, and the double refraction is negative. For sodium light. $\alpha = 1.8236$, $\beta = 1.8642$, $\gamma = 1.8736$, giving $\gamma - \alpha = .050$. The macro-axis is the acute bisectrix, $Vy = 25°$ 18′.

A specimen of hortonolite from Monroe, N. Y., was also investigated. The table given below exhibits at a glance the effect of the iron on the optical characters of the chrysolite-fayalite group:

[5] Am. Jour. Sci., CLI, pp. 129–135, Feb., 1896.

	% FeC	2 V (over ∝)	β
Fayalite Rockport,	68.1	49° 50′	1.864
Hortonolite Monroe,	47.3	69° 24′	1.791
Chrysolite, Auvergne,	13.0	89° 36′	1.692
Chrysolite, Vesuvius,	12.6	89° 42′	
Chrysolite, Hawaii,	10.3	91° 2′	
Chrysolite, Egypt,	9.2	91° 19′	1.678
Chrysolite, N. M.,	8.6	91° 24′	
Chrysolite, Unknown,	?	92° 14′	1.678
Chrysolite, East Indies,	?	92° 45′	1.670
Forsterite, Vesuvius,	2 (?)	93° 50′	1.657

At about 12% FeO, therefore, the optical character changes from positive to negative.

Rhodophosphite and Tetragophosphite.—The rare mineral locality at Horrsjöberg in Wermland, Sweden, is the source of these two new minerals recently described by Igelström.[6] The rhodophosphite occurs in large quantities in layers reaching a thickness of 2½ feet, so that it can be mined profitably. At one locality it is found in the form of hexagonal prisms. From the partial analysis, it appears to be chiefly a calcium phosphate, with considerable quantities of ferrous iron and manganese, also chlorine, fluorine, and sulphuric acid. The formula proposed is $20\ (RO)_3\ P_2O_5 + 4\ (Ca\ Cl_2, CaF_2) + Ca\ SO_4$, where R = Ca, Mn, Fe, or Mg. The mineral is allied to svanbergite.

Tetragophosphite occurs in "four-sided" plates, or as a coating on the containing cyanite-damourite rock. The two analyses are:

P_2O_5	36.92	33.64
Al_2O_3	40.00	41.81
FeO, MnO	9.51	9.51
MgO, CaO	7.50	6.74
H_2O	5.96	8.30

These lead to the formula $(Fe, Mn, Mg, Ca)_3\ P_2O_8 + (Al_2O_3)_3\ P_2O_5 + 3H_2O$. It is somewhat lighter blue than lazulite, which it seems here to replace. The "Gusblått-phosphat" (light blue phosphate) from the Westana Mts., Prov. Skåne, Sweden, analyzed by Blomstrand in 1868 seems to be undoubtedly the same mineral. He assigned the formula $(Ca, Mg)_3\ P_2O_8 + (Al_2O_3)_3\ P_2O_5 + 3H_2O$.

[6] Zeitschr. f. Kryst., XXV, pp. 433–436, 1895.

Miscellaneous Notes.—Von Zeynek[7] notes the occurrence of sulphur deposited in the canals carrying 1,000,000 gallons of water per day from the hot springs at Warasdin-Töplitz in Croatia.—Rohrer[8] gives results of two very careful analyses of hematite from Elba. The average of the two is as follows: SiO_2 .49, Fe_2O_3 98.60, CaO .42, MgO .74, total 100.25.—In an article on the contact of minerals of the Adamello Group of mountains in South Tyrol, Salomon[9] gives a detailed discussion of the Wernerite from Breno, with much of the literature relating to that mineral.—Duparc and Stroesco[10] have recorded the results of their observations on the crystalline form and optical behavior of thymoquinone and eleven of its derivatives.—Gentil[11] describes the occurrence of large bundles of yellowish-white somewhat altered sillimanite needles in pegmatite from Algeria. Veins of albite and plates of muscovite are also mentioned. The same author[12] makes a note of thomsonite, stilbite and analcite from an altered basic volcanic rock occurring near Dellys in the province of Algiers.—De Gramont[13] is led by the observation of the electric spark between fragments of certain minerals which are good conductors of electricity, to a study of the spectra of the sparks thus produced. This method promises to be useful for the rapid determination of certain minerals, and for the detection of included substances which are present only in traces. The lines of the non-metallic, as well as of the metallic elements may be observed. De Gramont also describes the apparatus used by him, and gives the details concerning the spectra obtained from air from twenty-four of the elements, and from about a hundred minerals.—Termier[14] calls attention to the two forms of the dimorphous substance PbO. After discussing the optical and crystallographic properties of the orthorhombic modification, he shows that its crystals are grouped to imitate a higher symmetry. PbO is, therefore, a good example of a substance which not only shows pseudo-symmetry by the grouping of the separate crystals, but also appears in a second form in which the *molecular* grouping follows an allied higher symmetry.—Gonnard,[15] in an article on French

[7] Tscherm. Mitth., XV, p. 192, 1895.
[8] Tscherm. Mitth., XV, pp. 184–187, 1895.
[9] Tscherm. Mitth., XV, pp. 159–183, 1895.
[10] Bull. Soc. Fr. Min., XVIII, pp. 126–141, 1895.
[11] Bull. Soc. Fr. Min , XVIII, pp. 170–171, 1895.
[12] L. c., p. 374.
[13] Bull. Soc. Fr. Min., XVIII, pp. 173–373, 1895.
[14] Bull. Soc. Fr. Min., XVIII, pp. 376–380, 1895.
[15] Bull. Soc. Fr. Min., XVIII, pp. 382–390, 1895.

siderites, adds to the thirteen forms of that mineral previously known the three rhombohedra $(03\bar{3}2)$, $(30\bar{3}4)$, and $(10\bar{1}2)$.—Termier and Richard[16] conclude from their study of crystals of $Ca_4P_2O_9$ occurring in the slags of the iron works at Kladno, that they are pseudo-orthorhombic, composed of monoclinic lamellæ. Measurements of the apparently orthorhombic form agree well enough with those of Miers to show that both had to deal with the same substance. The specific gravity is 2.93–3.1, mean index of refraction, about 1.64. For red light, 2 V = 20° (?), and for blue light it is about 40°.—O. Nordenskiöld[17] finds edingtonite from Bôhlet, Sweden, to be orthorhombic hemihedral instead of tetragonal hemihedral, as previously supposed. Sp. G. = 2.776, plane of optical axes = 010, negative bisectrix parallel to the vertical axis, 2 V for lithium light = 52° 47′, for sodium = 52° 55, and for thallium 53° 10′. The indices of refraction for the above kinds of light are also determined. The mean index for sodium light is 1.5492, and the double refraction is .016. In conclusion, the similarity of form with that of mesotype is shown by the axial ratios:

Edingtonite a : b : c = .9872 : 1 : .6733

Mesotype a : b : 2c = .9785 : 1 : .7072

—Goldschmidt[18] figures and describes a projection goniometer by means of which the position of crystal faces is projected directly upon paper, thus doing away with the reading of angles and with trigonometrical computation. The instrument seems to be in many ways convenient, but does not give the highest degree of accuracy. A contact goniometer of similar action is also briefly mentioned.

PETROGRAPHY.[1]

Geology of Point Sal, California.—The geology of Point Sal, the extreme northwestern corner of Santa Barbara County, California, has been carefully worked out by Fairbanks[2] with special reference to the igneous rocks found there. The sedimentary rocks constituting the point and the adjacent country are of miocene or later age. They

[16] Bull. Soc. Fr. Min., XVIII, pp. 291–295, 1895.

[17] Bull. Soc. Fr. Min., XVIII, pp. 395–398, 1895.

[18] Zeitschr. f. Kryst., XXV, pp. 538–560, 1895.

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Bull. Dep. Geol. Univ. of Cal., Vol. 2, p. 1.

comprise volcanic ashes, gypsiferous clays and bituminous shales, the last named of which were regarded by Lawson as tuffs. The present author declares them to be organic deposits. The igneous rocks which penetrate these beds are all basic. They include gabbros, peridotites, basalts, diabases and rocks similar to those heretofore described as analcite diabases. These latter are all now considered by the author as representing the otherwise practically unknown type of the teschenites. The augitic variety of this rock has the general structure of the diabases, in which are large poikilitic plates of augite. Between the diabasic constituents are polyhedral grains of analcite, and, in what appear to have been cavities in the rock-mass, are little groups of crystals and crystalline masses of the same mineral. The plagioclase in the rock is all zonal with nuclei of labrodorite surrounded by concentric zones of a more and more acid feldspar, the peripheral one being albite. An analysis of a coarse grained specimen gave:

SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	K_2O	Na_2O	P_2O_5	Ign.	Total
49.61	19.18	2.12	5.01	10.05	4.94	1.04	5.62	.27	3.55	=101.39

which corresponds very nearly to 43.3 per cent feldspar, with a density of 2.57, 32.3 per cent augite, 20 per cent analcite, 4 per cent magnetite and .04 per cent apatite. All of the analcite is supposed to be an alteration product of nepheline.

The basalts of the region include two types. One is the usual variety and the other an amygdaloidal and spheroidal variety that is intruded by diabases and diabasic gabbros. These last named rocks grade into one another. Both contain hornblende, some of which is regarded as secondary and some as primary. In addition to the diabasic-gabbros there are others associated with peridotites (and serpentines) in such a manner that both rocks are regarded as differentiated products of the same magma. The gabbro is sometimes massive. At other times it is possessed of a gneissic structure, often attended by a striping produced by the alternation of augitic and feldspathic bands. The structure is concluded, after study, to be the result of stretching.

Among the other basic rocks identified in the gabbro-peridotite complex are anorthosites, diorites, norites, lherzolites, picrites, saxonites, wehrlites, dunites and pyroxenites. Each type is well described and a discussion of the banding noticed in many of them is given in some detail.

Leucite-Basanites of Vulcanello.—After studying carefully the rocks on Vulcanello in the Lispari Islands, Bäckström[3] concludes

[3] Geol. För. i Stockh. Förhanl., XVIII, p. 155.

that the greater portion of them are leucite-basanites. They all contain phenocrysts of augite, labradorite, olivine and magnetite in a groundmass which is sometimes a holocrystalline aggregate of oligoclase, orthoclase, leucite and magnetite, and at other times of numerous leucites, small augites and iron oxides in a glassy matrix. The rocks are regarded as effusive types of lamprophyres (minettes or kersantites) a supposition which is the more probable from the fact that the effusives in the Lipari province are mainly feldspathic basalts, andesites, liparites and trachytes. Biotite and leucite are thought to be complementary minerals—the former separating from a siliceous magma under considerable pressure, and the latter from a magma of the same composition under surface pressure, under conditions favorable to the escape of the mineralizers fluorine and water. Leucite is not confined to rocks rich in potash, nor is it necessarily characteristic of these. Its place may often be taken by biotite.

A Squeezed Quartz-Porphyry.—A squeezed quartz-porphyry is described by Sederholm[4] as occurring at two places in the Parish of Karvia in Province Abo, Finland. In both it appears as dykes cutting granite. The rock consists mainly of microcline phenocrysts to which are often added growths of new microcline in optical continuity with the original crystals, phenocrysts of an acid plagioclase surrounded in many cases by microcline substance and quartz phenocrysts in a groundmass of orthoclase and quartz. The twinning of the microcline is more largely developed around quartz enclosures in the phenocrysts and near quartz veins than elsewhere in the crystals. The porphyritic quartzes occasionally retain their dehenhedral contours, but usually they are much deformed in outline and in their optical characteristics. Often the quartzes are so shattered that they now constitute lenticular areas of a quartz mosaic. The structure of the groundmass is in several types. In the most important one it consists of a micropegmatite of orthoclase and quartz containing shreds of chlorite, which in some cases are distributed so as to exhibit a fluidal arrangement. The granite through which the porphyry cuts is a coarse grained porphyritic variety composed of oligoclase, biotite and hornblende. On the contact with the dyke rocks it is crushed and much epidote is developed in it. Under the microscope it presents the usual aspects of a dynamically metamorphosed rock. In his discussion concerning the name to be applied to the porphyry, the author quotes from a letter by Dr. Williams in which the prefix 'apo' is defined as signifying that the rock

[4] Bull. Com. Geol. d'Finlande, No. 2, 1895.

to which it refers has been changed from its original character through devitrification.

Mica-Syenites at Rothschonberg.—Two dykes of mica-syenite cut the phyllite formation near Rothschönberg, Saxony, producing in the neighboring rocks contact metamorphism. One of the dykes weathers spheroidally, and in the kernels of the spheroids fresh material for study was afforded Henderson,[5] who found the rock to be composed of orthoclase, plagioclase, quartz, biotite, apatite and several accessory components. The feldspar and quartz both occur in grains and in crystals, the biotite in flakes. An analysis of the rock gave the figures below (I).

The second occurrence differs little from the first. Muscovite is present as well as biotite, otherwise the two rocks are practically alike in mineral composition. Its chemical composition is shown in (II).

	SiO_2	Al_2O_3	Fe_2O_3	CaO	MgO	K_2O	Na_2O	H_2O	CO_2	S	Total
I.	61.40	16.66	7.46	2.08	3.65	2.93	4.75	.76	1.54	.20	=101.43
II.	57.63	16.47	5.37	5.25	4.44	3.12	5.15	.45	2.14	.95	=100.97

The structure of both rocks was panidiomorphic, although the development of secondary quartz renders them now hypidiomorphic. They are syenitic aplites. In the neighboring phyllites new biotite has been abundantly developed and hornblende has been produced in some quantity. The free silica which is abundant in the unaltered phyllites has become combined with metallic elements in the altered forms. While the percentage of silica in specimens taken at 2 meters and 11 meters from the contact and at the contact is the same, the free quartz in the first is 43.38 per cent of the rock's mass, in the second 38.94 per cent and in the third 34.06 per cent.

GEOLOGY AND PALEONTOLOGY.

Cambrian Rocks of Pennsylvania.—During the years 1892 and '93, Mr. Walcott made an examination of the rocks of Cambrian southeastern Pennsylvania for the purpose of determining whether the lower quartzites with their superjacent limestones were of the same geologic age, in the areas included between the Potomac and Susquehanna and the Delaware and Susquehanna Rivers. The results of his work are published in bulletin form by the U. S. Geological Survey.

[5] Zeits. d. geol. Ges., XLVII, p. 534.

Mr. Walcott began the investigation in York County, where he determined the stratigraphic position of the Chickies quartzites and the York shales which are subjacent to the Lancaster (York Frazer) limestone. Paleontologic evidence shows them to be of Lower Cambrian age. The fauna of the main body of the limestone of York County, as shown by collections from three separate localities, is Cambrian. These localities indicate respectively, an Upper Olenellus zone, a horizon between the Lower and Middle Cambrian, and a lower horizon of the Middle Cambrian.

The discovery of Lower Cambrian fossils in the compressed syncline of limestone in Lancaster County, south of Columbia, indicates that the limestone on the west side of the river is of the same geologic age, and that the shales and schists beneath it are of Lower Cambrian age.

Mr. Walcott states that in York County there is no sedimentary rock other than the mesozoic new red sandstone—of later age than the Cambrian, unless it be the Peach Bottom slates and chlorite-schists of the southeastern corner of the country. He also thinks it probable from the closely related structure of Lancaster County that all the Lancaster limestones will fall within the Cambrian, unless it be some portions of the upper series, which may pass into the Ordovician. He applies this generalization to the entire extension of this series of limestone northeastward to the Delaware.

All of the quartzites that have been referred to the Potsdam necessarily fall into the Lower Cambrian, as they are beneath the limestones.

The South Mountain chain, as stated by Professor Lesley and Dr. Frazer, consists of two groups of rocks, a quartzite and an orthofelsite series, the latter being considered the superior series. Mr. Walcott's investigations lead him to a different interpretation of the geologic structure of the mountain and the relations of the rocks composing it. He finds that the "orthofelsite" is in reality the lower series, and that the complicated structure of the mountain arises "partly from folding, but more largely from the westward thrusts of masses of strata along the lines of fault of a low hade. This westward thrusting on the fault plane, complicated by previous folding of the strata, leaves masses of the subjacent, pre-Paleozoic rocks resting in various places on different members of the Lower Cambrian series, and also appears to interbed the quartzites and schists of the Cambrian in the schists, eruptives, etc., of the Algonkian."

The following are the concluding remarks in the section on Correlation :

"The discovery of the Olenellus or Lower Cambrian fauna in the Reading sandstone practically completes the correlation of the South Mountain, Chickies and Reading quartzites, and establishes the correctness of the early correlations of McClure, Eaton, Emmons and Rogers. They all considered the basal quartzite as the same formation from Vermont to Tennessee; and the discoveries of recent years have proved that the basal sandstones of Alabama, Tennessee and Virginia (Chilhowee quartzite); Maryland, Pennsylvania and New Jersey (Reading quartzite); New York and Vermont (Bennington quartzite), were all deposited in Lower Cambrian time, and that they contain the characteristic Olenellus fauna throughout their geographic distribution. The superjacent limestones carry the Olenellus fauna in their lower portions in northern and southern Vermont, eastern New York, New Jersey and Pennsylvania. To the south of Pennsylvania the lower portions of the limestones appear to be represented by shales, and the Upper and Middle Cambrian faunas are found in the lower half of the Knox dolomite series of Tennessee, and they will probably be discovered in the same series in Virginia and Maryland when a thorough search is made for them. The same may be predicted, but with less assurance, for the northern belt of limestone crossing Pennsylvania into New Jersey as the limestones between the Olenellus zone and the Trenton zone represent the intervals of the Middle and Upper Cambrian and the Lower Ordovician, or the Calciferous and Chazy zones of the New York section."

Nothing was discovered upon which could be based a line of demarcation between the Cambrian and Ordovician linestones in the series under discussion. The division is still an open question to be decided by future revelations of lithologic and paleontologic characters. (Bull. U. S. Geol. Surv., No. 134, Washington, 1896.)

Structure of Uintacrinus.—Since Uintacrinus was first made known by Grinnell, some twenty years ago, little notice has been taken of the form. Of late, however, special interest in the type has been revived, and the form comes in for consideration in several important articles. Among them are W. B. Clark's review of the Mesozoic Echinodermata of the United States[1], in which all known material is described, and the structure amply illustrated by figures. Shortly after Williston and Hill[2] gave some "Notes on *Uintacrinus socialis*" as found in Kansas. Still more recently Bather[3] has gone over all the

[1] U. S. Geol. Sur., Bull. 97, pp. 21–24, 1893.

[2] Kansas Univ. Quarterly, Vol. III, pp. 19–21, 1894.

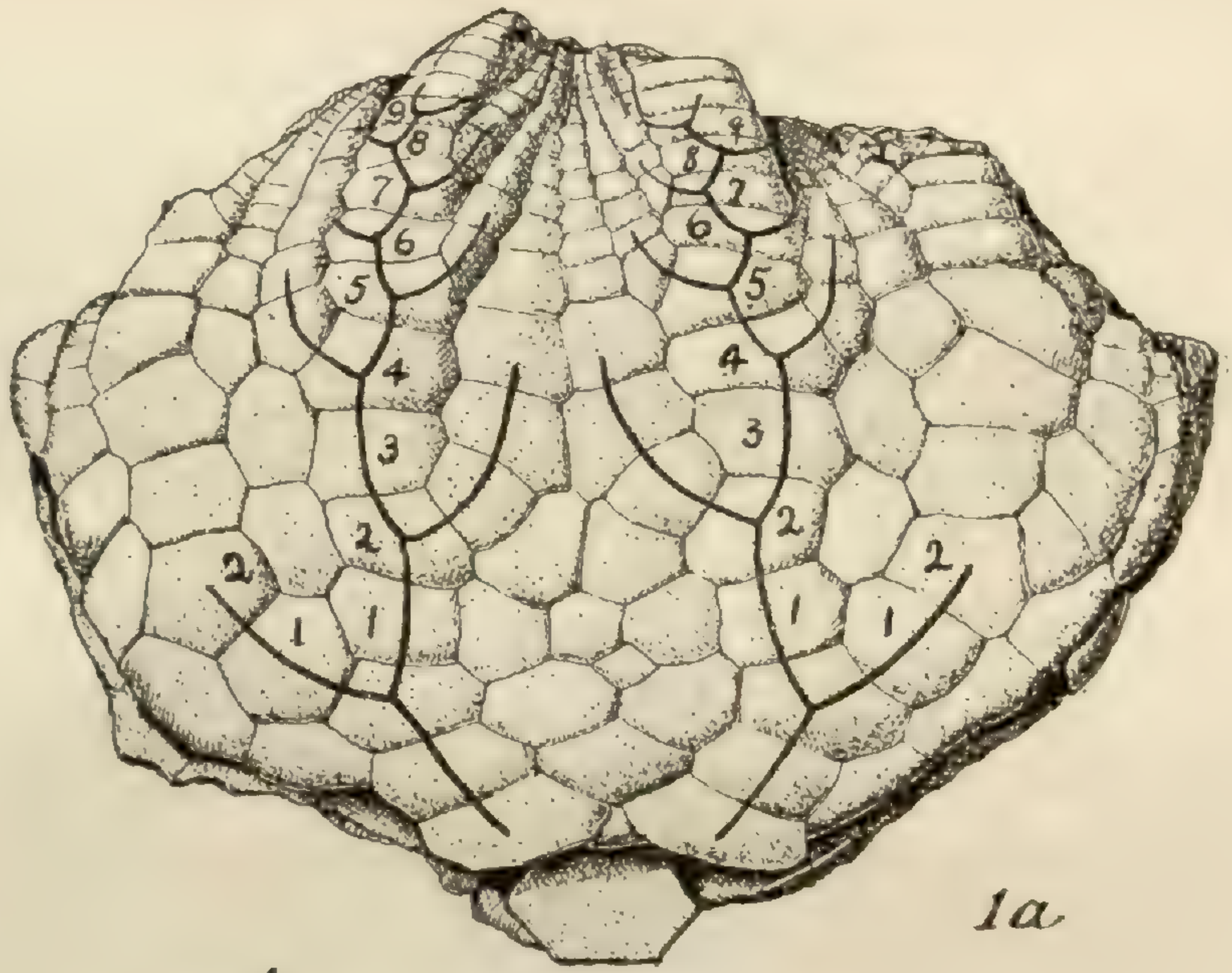
[3] Proc. Zool. Soc., London, 1895, pp. 974–1004, 1896.

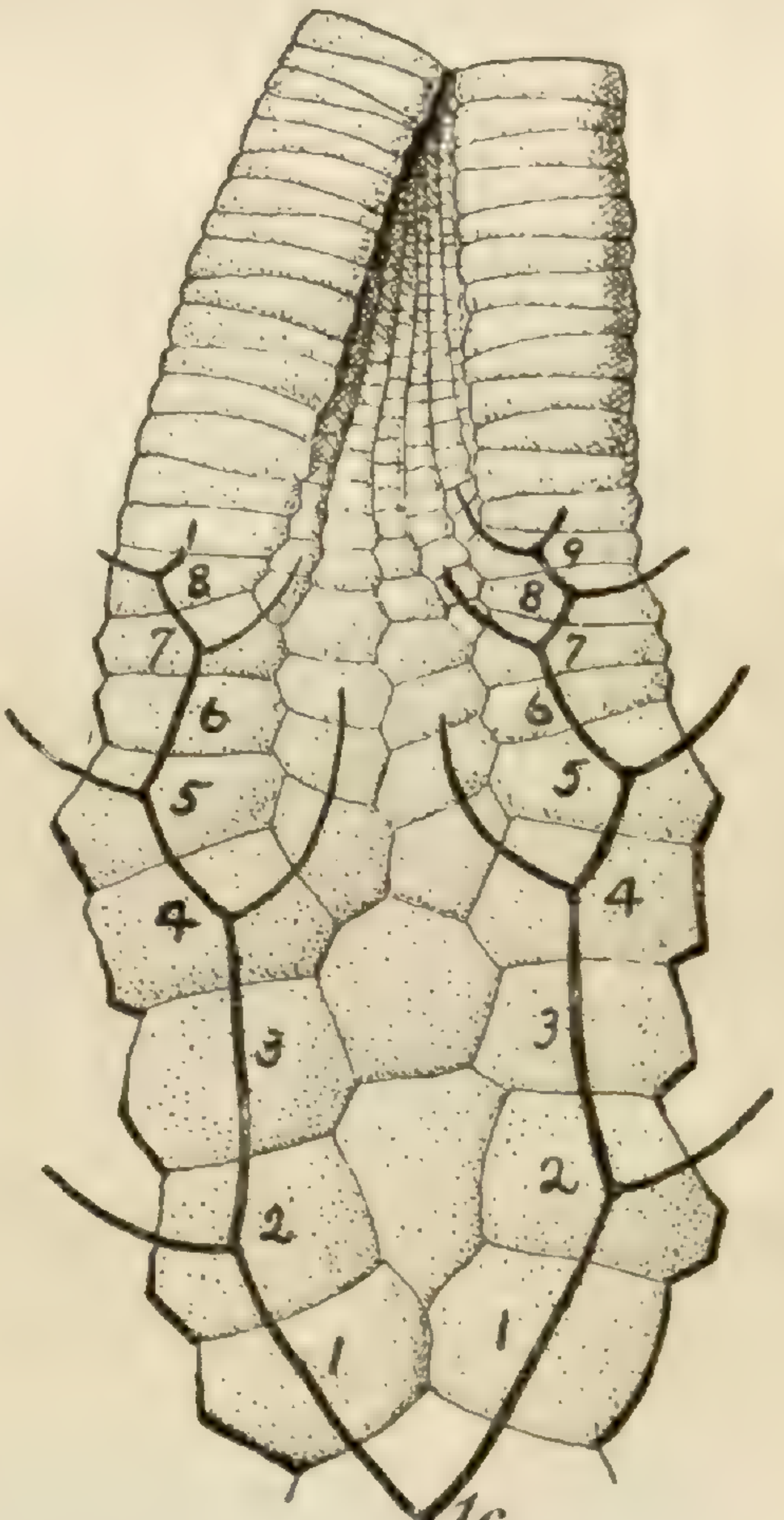
available data, and has made the type of Uintacrinus the subject of a a special morphological study. In this treatment the previous work is briefly referred to; but some of it receives criticism that it does not appear to deserve, particularly since the foundation of most of the adverse comments lies not in any material error in the work referred to, but in what is manifestly a clear misinterpretation or hasty perusal of that work. It is to certain of these points in the structure of the form that attention is directed in the present note. In making the correction, however, it is not with the idea of reflecting on Mr. Bather's paper as a whole, for it is one of the most excellent contributions to echinoderm morphology that has yet appeared. The original figures in question from Bulletin 97 of the U. S. Geological Survey are herewith reproduced (Plate XV) in order to make more intelligible the exact points under consideration.

In the memoir mentioned considerable space is occupied in criticising a recent account of the species; but most, if not all of the objections urged against Doctor Clark's work, are certainly more imaginary than real. Professor Clark's figures come in for special condemnation as violating the fundamental law of the alternation of the pinnules. As a matter of fact his plate which is reproduced in the Proceedings as Plate LVI, to point out the alleged errors, not only shows that the accompanying statements are not true, but that in all three figures there is strict alternation of the pinnules in every case.

The general law in the pinnulation of the genus Mr. Bather states as follows: $IIBr_1$ none, $IIBr_2$ outer, $IIBr_3$ none, $IIBr_4$ inner, $IIBr_5$ outer, $IIBr_6$ none, $IIBr_7$ inner, $IIBr_8$ outer, $IIBr_9$ none. Two of his ten specimens differed from this general rule: one showing $IIBr_6$ inner, $IIBr_7$ none, $IIBr_8$ outer, $IIBr_9$ inner; and the other $IIBr_5$ none, $IIBr_6$ outer, IIB_7 inner, $IIBr_8$ none, $IIBr_9$ outer. He makes out the formula for the Clark figure 1a to be $IIBr_5$ outer, $IIBr_6$ none, $IIBr_7$ outer. The real formula for this is $IIBr_5$ outer, $IIBr_6$ inner, $IIBr_7$ outer, $IIBr_8$ inner, $IIBr_9$ outer. This appears clearly indicated in the figure, and Mr. Bather's statements that $IIBr_6$ has no pinnule is certainly a typographical error, for it cannot be that he mistook the rough, broken and highly raised edge of the brachial row of plates, with its deep shadow, for a suture line connecting with the first inside pinnule. The figure is of a somewhat crushed and distorted specimen, and the perspective is, perhaps, not as good as it might be. Whether or not it is the same as that figured by Meek (Bather's figure 2) is not known; but if the two are the same, the difference in the sketches are not very great nor radical, and certainly not as contradictory as Mr. Bather would have

PLATE XV.





Ch. Keyes, Del.

Keyes on Uintacrinus.

us believe. The only real difference is that in Meek's figure there is above $IIBr_5$ a non-pinnulated brachial separating two pinnule-bearing arm-plates. The same extra plate is shown in figure 1c. The intercalation of a non-pinnulated plate of this kind is of common occurrence among crinoids, and should not excite any wonderment, let alone adverse criticism. In Uintacrinus this is of frequent occurrence, not only in different specimens but also in the different arms of the same individual. Mr. Bather himself shows that two out of the ten examples which he examined do not agree with his general formula. If his observations could have been extended it might have been found that one in three is just about the proportion of the individuals that do not agree in this respect. It might be added parenthetically that of 200 specimens of Uintacrinus that have been personally examined, fully one-half of them appear to vary in a similar way in the mode of pinnulation.

Granting then that Meek's original figure is correct in every detail and that figure 1a of Professor Clark's plate represents the same specimen, the differences are practically immaterial. Professor Clark's work not only does not violate the fundamental law of pinnulation but is essentially correct so far as typifying the species. The very object of adding an enlarged view of another specimen (figure 1c) was to make more clear the arrangement of the pinnules, since the convexity of the principal specimen and its distortion somewhat misrepresented these points. The same may be said of $IIBr_9$ in figure 1c. Usually from $IIBr_7$ on, the pinnules are turned in or are covered by the matrix if the specimen is not carefully cleaned, and consequently do not show from above. That $IIBr_9$ should be visible on one arm and not on another is not strange, and not necessarily incorrect.

Regarding Mr. B. H. Hill's diagram the formula given by Mr. Bather is probably correct for the figure; but, as he has stated, it is difficult to decide the question owing to the "rather peculiar mode of representation." It seems hardly possible that this diagram correctly represents the pinnulation of the specimen from which it was taken, and the entire disagreement of the corresponding plate faces when separated would further indicate that the figure is, as stated by Mr. Bather, "hopelessly incorrect," or illustrates a new species.—C. R. Keyes.

Geological News.—Paleozoic.—From data recently compiled, Prof. C. R. Keyes estimates the total maximum thickness of the Paleozoic rocks in the middle part of the central Mississippi basin, that is, in the neighborhood of the Missouri River, between Kansas City and the Iowa

boundary, to be 3000 feet. This differs considerably from the earlier estimates. (Amer. Geol., 1896.)

Bulletins No. 6 and No. 7, (1895) and No. 8, (1896) of the Illinois State Museum of Natural History contain descriptions of new Paleozoic Echinodermata, by S. A. Miller and Wm. F. E. Gurley. One new family (Thalamocrinidæ) and five new genera (Sampsonocrinus, Emperocrinus, Shumardocrinus, Thalamocrinus, Indianocrinus) are defined. In all, 156 species are described and figured.

Mesozoic.—According to F. H. Knowlton, the fossil flora of Yellowstone Park represents three distinct stages. The first, or older flora, from the acid rocks embraces 79 forms; the second, or intermediate flora, has 30 species; and the third, or younger flora, comprises 70 forms. The author refers the first stage division to the Ft. Union or lower Eocene; the second is regarded as Miocene, but older than the Auriferous Gravels; and the third is probably of the same age as the Auriferous Gravels of California, that is, Upper Miocene. (Amer. Journ. Sci., July, 1896.)

A new fossil plant, *Salvinia elliptica*, is described and figured by Prof. Hollick. The new species is from the Upper Cretaceous of Washington State. (Bull. Torrey Botan. Club, Vol. 21, 1894.)

BOTANY.[1]

Botany at Buffalo.—In August (21 to 28) there were three botanical meetings held in Buffalo, as follows:

The Botanical Society of America met on Friday and Saturday in the High School, with eleven members in attendance. C. H. Peck, of Albany, and B. T. Galloway, of Washington, were elected to membership. The question of the desirability of a winter meeting was discussed and referred to the Executive Committee. Appropriate resolutions regarding the death of M. S. Bebb, a member, were adopted. The address of the retiring President, William Trelease, on "Botanical Opportunity," was given in open session on Friday evening. This will be printed in full in *Science* and the *Botanical Gazette*, and will also be distributed in pamphlet form by the Secretary. The following papers were accepted for presentation:

The Philosophy of Species-making. By L. H. Bailey.

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

Some Characteristics of a Fresh-water Insular Flora. By Conway MacMillan.

Some Problems in Sporophyll Transformation among Dimorphic Ferns. By G. F. Atkinson.

A Species of Eleocharis New to North America. By N. L. Britton.

In the election of officers for the ensuing year, the following were chosen: John M. Coulter, Chicago, President; Charles S. Sargent, Brookline, Vice-President; Arthur Hollick, Brooklyn, Treasurer; Charles R. Barnes, Madison, Secretary; Benjamin L. Robinson, Cambridge, and Frederick V. Coville, Washington, Councillors.

The Botanical Section (G) *of the American Association for the Advancement of Science.* An unusually large number of papers were read before the Section, and it is not too much to say that in point of importance they fully maintained the high average of recent years:

1. The Relation of the Growth of Leaves to the CO_2 of the Air. By D. T. MacDougal.

2. Directive Forces Operative in Leaf Rosettes. By R. N. Day.

3. On *Crataegus coccinea* and its segregates. By N. L. Britton.

4. The Distribution of the species of *Gymnosporangium* in the South. By L. M. Underwood and F. S. Earle.

5. Morphology of the *Canna* Flower. By L. H. Bailey.

6. A Comparison of the Flora of Erie Co., Ohio, with that of Erie Co., New York. By E. L. Moseley.

7. The Significance of Simple and Compound Ovaries. By C. E. Bessey.

8. On the Bacterial Flora of Cheddar Cheese. By H. L. Russell.

9. The Terminology of Reproduction and Reproductive Organs. By C. R. Barnes.

10. A Comparative Study of the Development of some Anthracnoses in Artificial Cultures. By Bertha Stoneman.

11. The Development of Vascular Elements in the Primary Root of Indian Corn. By W. W. Rowlee.

12. Some Remarks on Chalazogamy. By J. M. Coulter.

13. The Habits of the Rarer Ferns of Alabama. By L. M. Underwood.

14. On the Stem Anatomy of Certain *Onagraceae*. By Francis Ramaley.

15. The Point of Divergence of Monocotyledons and Dicotyledons. By C. E. Bessey.

16. Notes on the Pine Inhabiting Species of *Peridermium*. By L. M. Underwood and F. S. Earle.

17. Reaction of Leaves to Continual Rain-Fall. By D. T. MacDougal.

18. Studies in Nuclear Phenomena, and the Development of the Ascospores in Certain *Pyrenomycetes*. By Mary A. Nichols.

19. The Stigma and Pollen of *Arisaema*. By W. W. Rowlee.

20. Notes on the Genus *Amelanchier*. By N. L. Britton.

21. Remarks on the Northern Species of *Vitis*. By L. H. Bailey.

22. On the Formation and Distribution of Abnormal Resin Ducts in Conifers. By Alex. P. Anderson.

23. The Development of the Cystocarp of *Griffithsia bornetiana*. By Arma A. Smith.

24. Notes on the Allies of the Sessile *Trillium*. By. L. M. Underwood.

25. On an Apparently Undescribed *Cassia* from Mississippi. By C. L. Pollard.

26. A Bacterial Disease of the Squash-Bug (*Anasa tristis*). By B. M. Duggar.

27. What is the Bark? By C. R. Barnes.

28. Embryo-Sac Structures. By J. M. Coulter.

29. Some *Cyperaceæ* New to North America, with Remarks on Other Species. By N. L. Britton.

30. Grasses of Iowa. By L. H. Pammel.

31. Ceres-Pulver: Jensen's New Fungicide for the Treatment of Smut. By W. A. Kellerman.

32. On the Cardamines of the *C. hirsuta* group. By N. L. Britton.

33. The Relation Between the Genera *Polygonella* and *Thysanella*, as Shown by a Hitherto Unobserved Character. By John K. Small.

34. An Apparently Undescribed Species of *Prunus* from Connecticut. By John K. Small.

35. The Flora of the Summits of King's Mountain and Crowder's Mountain, North Carolina. By John K. Small.

36. Parthenogenesis in *Thalictrum fendleri*. By David F. Day.

37. Notes on the Order *Pezizineæ* of Schröter. By Elias J. Durand.

38. What Should Constitute a Type Specimen? By S. M. Tracy.

39. Rheotropism and the Relation of Repose to Stimulus. By F. C. Newcombe.

40. Some Adaptation of Shore Plants to Respiration. By Herman von Schrenk.

41. The Mechanism of Curvature in Tendrils. By D. T. MacDougal.

42. A Contribution to Our Knowledge of Turgor. By E. B. Copeland.

The Botanical Club. This vigorous organization fully justified its existence again. About forty papers were read. Many of these, of course, were notes, but others were of considerable value. Among the more important of these were the following:

The Distribution of *Phoradendron flavescens*, *Polypodium polypodoides* and *Bignonia crucigera* in Ohio. By W. A. Kellerman.

A Method of Distributing Fungi in Pure Cultures. By L. R. Jones.

Notes on Some Mosses. By Mrs. E. G. Britton.

Notes on *Iris*. By David F. Day.

An Improved Paraffin Bath. By F. C. Newcombe.

Notes on Oaks. By W. W. Rowlee.

Some Notes on Potato-Leaf Fungi. By L. R. Jones.

A Method of Preventing Condensation of Water in Culture Dishes. By H. L. Russell.

Notes on the Flora of Colorado Springs, Colorado. By Charles E. Bessey.

On a Species of *Epipactis*. By Elias J. Durand.

A Report Upon the National Herbarium. By C. L. Pollard.

Schizaea pusilla from Newfoundland. By Mrs. E. G. Britton.

Photosyntax *vs* Photosynthesis. By C. R. Barnes.

The Distribution of *Pinus ponderosa* in Nebraska. By Charles E. Bessey.

Some Curious Sunflowers. By J. F. Cowell.

Notes on Species of *Mnium*. By Mrs. E. G. Britton.

The Canyon Flora of the Plains. By Charles E. Bessey.

The Turgor of Mosses. By E. B. Copeland.

A Simple Apparatus for Spraying and Infecting Plants. By A. P. Anderson.

The Structure of Pseudoparenchyma in Higher Fungi. By Elias J. Durand.

Note on the Hosts of *Comandra umbellata* and *C. pallida*. By Herman von Schrenk.

The Submerged Leaves of *Salvinia natans*. By Conway MacMillan.

Nuclear Budding in *Cypripedium*. By Conway MacMillan.

An Unusual Adaptation of Conifers for Wind Protection. By Conway MacMillan.

Some Plants New to the Rochester Flora. By Florence Beckwith.

The Committee on Nomenclature made a report recommending that:

(1) A list of North American Pteridophyta and Spermatophyta be prepared for publication.

(2) A supplement be prepared to the present List of Pteridophyta and Spermatophyta of the Northeastern United States.

(3) The Rochester-Madison Rules be republished with annotations and explanations.

The officers for the next year are: President, S. M. Tracy, Agricultural College, Miss.; Vice-President, L. R. Jones, Burlington, Vt.; Secretary-Treasurer, E. Burgess, New York.

A New Manual of Systematic Botany.[2]—It is many years since American botanists have had the pleasure of examining a new manual of systematic botany designed for use in the northeastern States. We have had new editions of old books, but, unfortunately for scientific progress, through a remarkable misconception of the duties of executors, these editions were new mainly in type and binding, the additions and modifications having been purposely reduced to a minimum. It has been a matter of profound regret on the part of many of the friends and admirers of Dr. Gray that his books should receive such a treatment as to prematurely relegate them to the list of antiquated and obsolescent works. A new manual is, therefore, of peculiar interest at the present time, and this interest is enhanced by the fact that it comes from the scientific home of the older botanist, Torrey.

The volume before us is the first of the three volumes which will include descriptions and figures of all the ferns and flowering plants of the northeastern States and Canada. It is in every way a new work—new in its plan, new in its descriptions, new in its illustrations. Volume I opens with an eight-page introduction which is historical and explanatory. Here we learn that more than 4,000 species will be included, and that nearly three-fourths of these will be figured for the first time. Discussions follow on the principles of classification of plants, and the systematic arrangement adopted in the work (Engler and Prantl's). The following quotations are useful and suggestive: "The Nineteenth Century closes with the almost unanimous scientific judgment that the order of nature is an order of evolution and development from the more

[2] *An Illustrated Flora of the Northern United States, Canada and the British Possessions* from Newfoundland to the Parallel of the Southern Boundary of Virginia, and from the Atlantic Ocean Westward to the 102d Meridian. By Nathaniel Lord Britton, Ph.D., Emeritus Professor of Botany in Columbia University, and Director-in-Chief of the New York Botanical Garden, and Hon. Addison Brown, President of the Torrey Botanical Club. Vol. I, Ophioglossaceae to Aizoaceae. New York: Charles Scribner's Sons. 612 pages, octavo, $3.00.

simple to the more complex." * * "Systematic arrangement should logically follow the natural order." * * "The sequence of families adopted fifty or seventy-five years ago has become incongruous with our present knowledge, and it has, for some time past, been gradually superseded by truer scientific arrangements in the later works of European authors." "The more simple forms are, in general, distinguished from the more complex, (1) by fewer organs or parts; (2) by the less perfect adaptation of the organs to the purposes they subserve; (3) by the relative degree of development of the more important organs; (4) by the lesser degree of differentiation of the plant body or of its organs; (5) by considerations of antiquity, as indicated by the geological record; (6) by a consideration of the phenomena of embryogeny. Thus, the Pteridophyta, which do not produce seeds and which appeared on the earth in Silurian time, are simpler than the Spermatophyta; the Gymnospermae in which the ovules are borne on the face of a scale, and which are known from the Devonian period onward, are simpler than the Angiospermae, whose ovules are borne in a closed cavity, and which are unknown before the Jurassic."

"In the Angiospermae the simpler types are those whose floral structure is nearest the structure of the branch or stem from which the flower has been metamorphosed, that is to say, in which the parts of the flower (modified leaves) are more nearly separate or distinct from each other, the leaves of any stem or branch being normally separated, while those are the most complex whose floral parts are most united."

"The sequence of families adopted by Engler and Prantl, in 'Natürlische Pflanzenfamilien' above referred to, has been closely followed in this book, in the belief that their system is the most complete and philosophical yet presented. The sequence of genera adopted by them has, for the most part, also been accepted, though this sequence within the family does not attempt to indicate greater or less complexity of organization."

The nomenclature is that of the well-known Rochester-Madison Rules of the Botanical Club of the American Association for the Advancement of Science, and, in order that the student may fully understand the matter, the rules are printed in full, with explanatory notes. As this work will at once become the standard botanical manual everywhere in this region for which it is designed, the revised nomenclature will soon be more familiar than the old which it is rapidly superseding.

The work proper opens with the family *Ophioglossaceae*, the lowest of the Pteridophyta, in which one species of *Ophioglossum*, and six of *Botrychium* are described and figured. Then follow *Osmundaceae* (3

species), *Hymenophyllaceae* (1 sp.), *Schizaeaceae* (2 sp.), *Polypodiaceae* (59 sp.), *Marsileaceae* (2 sp.), *Salviniaceae* (2 sp.), *Equisetaceae* (11 sp.), *Lycopodiaceae* (11 sp.), *Selaginellaceae* (3 sp.), and *Isoetaceae* (8 sp.).

The flowering plants, as we have been calling them, are here more correctly called seed-bearing plants (*Spermatophyta*), and are properly divided into two classes—*Gymnospermae* and *Angiospermae*, and the latter into the sub-classes *Monocotyledones* and *Dicotyledones*. As one turns over the pages, reading the full descriptions and comparing them with the excellent illustrations (which are always by the side of the descriptions) the conviction deepens that this book is one of the most important systematic works yet produced in this country. This is well illustrated in the treatment of the sedge and grass families which fill two hundred and sixty-six pages. Any one who has tried to puzzle out the genera and species of grasses and sedges will not have to be told of the great advantage which good figures will give to the student of these difficult families. It is not too much to say that no publication hitherto made has done so much to popularize the study of these plants as the one now before us. Of *Carex* alone two hundred and five species are figured!

We have not the space at our command to speak of the many changes in generic and specific limits with which this work will familiarize us. Nearly all of these have been known to specialists and those who have kept their eyes on the work of the German systematists, but to many the changes will come as novelties.

Among the minor matters may be mentioned the abandonment of the absurdity of calling families "orders," thus conforming to the usage in other departments of biological science. All family names (with a very few exceptions) are now made to end in *aceae*, a commendable practice which Dr. Gray used to insist upon. Throughout the work all diphthongs are printed in separate letters (*ae*, *oe*), and not in single characters (*æ*, *œ*), thus again conforming to the German usage. In the rules for pronunciation, a little easement is made for the use of the Roman pronunciation of botanical names, which we wish had been made a little more evident. We regret to see the use of feet, inches and lines still adhered to in this otherwise modern work. The metric units are so generally used in scientific books everywhere that we are surprised at this unexpected anachronism.

As we carefully study the beautifully-printed pages of this work, we are more and more impressed with its magnitude and importance. It will give renewed life and vigor to systematic botany, and doubtless will be the means by which many a student will be led to the study of

the more difficult families. It is so valuable in so many ways that we wish it would be made available to a greater number of students. We venture to suggest to authors and publishers that they bring out an edition *without the illustrations*, and printed on thin paper, with narrow margins. A single volume, portable edition of this admirable book, would greatly extend its usefulness.—CHARLES E. BESSEY.

ZOOLOGY.

The Heart of Some Lungless Salamanders.[1]—The recent literature of zoology has, perhaps, contained nothing more unexpected and startling than that certain adult salamanders are entirely lacking in those respiratory organs which, heretofore, have been deemed indispensable to the existence of animals so high in the zoological scale as the Amphibia. This total lack of lungs and branchiæ appears the more marvelous when we remember that they are absent in forms which lead a rather active and wholly terrestrial life, as well as in those of more or less purely aquatic habits.

Two questions are naturally suggested by this apparently aberrant condition of the respiratory organs. First, what structures or organs have taken on the functions of the lungs and branchiæ? and secondly, is there any modification in the form or structure of the heart which in any way may be correlated with the above mentioned peculiarities of these lungless forms?

The first of these two questions has been discussed to some extent by Prof. Harris H. Wilder, of Smith College, who first published an account of this apparently anomalous condition. He concluded that respiration was probably carried on by the skin and, perhaps, to some extent, by the mucosa of the intestine. Prof. Camerano has also published the results of some experiments upon two European forms which bear upon this same question. He believes that in these lungless forms respiration is effected in the bucco-pharyngeal cavity, and that the skin affords no efficient aid in the respiratory processes.

In a still later paper he discusses the subject further, and tries to account for the disappearance of the lungs. Of one aquatic species (of the genus Molge) he says: "The function of the lungs as hydrostatic organs, is very marked." "In the clearly terrestrial forms one would say that the diminution in importance of the function of the lungs as hydrostatic organs induces a retrogressive development of them while

[1] Read before the Amer. Assoc. Adv. Science, Aug. 24, 1896.

at the same time the importance of the bucco-pharyngeal respiration is increased."

It appears to the writer that Camerano's conclusions need to be tested by further experiments, especially the part referring to the respiration, for the area of the dermal surface far exceeds that of the bucco-pharyngeal cavity, and the skin is also very richly supplied with blood vessels which are so close to the surface that it would appear as if the gases of the blood and air might be readily interchanged. It is hoped that time will permit of some experiments on this point during the coming year.

As to the second question, whether there is any appreciable modification of the heart in these lungless salamanders, nothing whatever has been published.

It is the object of this paper to call attention to the fact that there is a difference in the heart of those salamanders that do not have lungs and those which do have them. So far as I have examined, it is possible to distinguish between the two forms by examining the heart alone.

In order that what is said on this point may be clearly understood by every one, and in order to bring out the differences between the two more sharply, if possible, I wish first to recall to mind the structure of the Amphibian heart and then contrast with it the relations as found in the heart of a lungless individual. We may take Huxley's description of the Amphibian heart as our standard of comparison. In his Anatomy of Vertebrates he says: "The heart presents two auricles, a single ventricle and a bulbus arteriosus. A venous sinus, the walls of which are rhythmically contractile, receives the venous blood from the body and opens into the right auricle. The left auricle is much smaller than the right and a single pulmonary vein opens into it." In regard to the septum of the auricles, he says that "it is less complete in Proteus, Siren and Menobranchus (Necturus) than in other Amphibia. In Menobranchus the septum is reduced to little more than a wide meshed network of branched muscular bands, and in Proteus the existence of a septum is doubtful."

The heart of our common Newt (Diemyctylus viridescens) Fig. 1 or of the large yellow-spotted salamander (Amblystoma punctatum), for examples, corresponds perfectly with Huxley's description. In both of these forms the auricular septum is perfectly complete, the cavities of the auricles being entirely separated, except just at the auriculo-ventricular aperature, at which point the two auricles communicate with each other to some extent.

In Necturus, the septum is more or less fenestrated and, according to Huxley, it is very incomplete in Proteus and Siren, but in all of the

PLATE XVII.

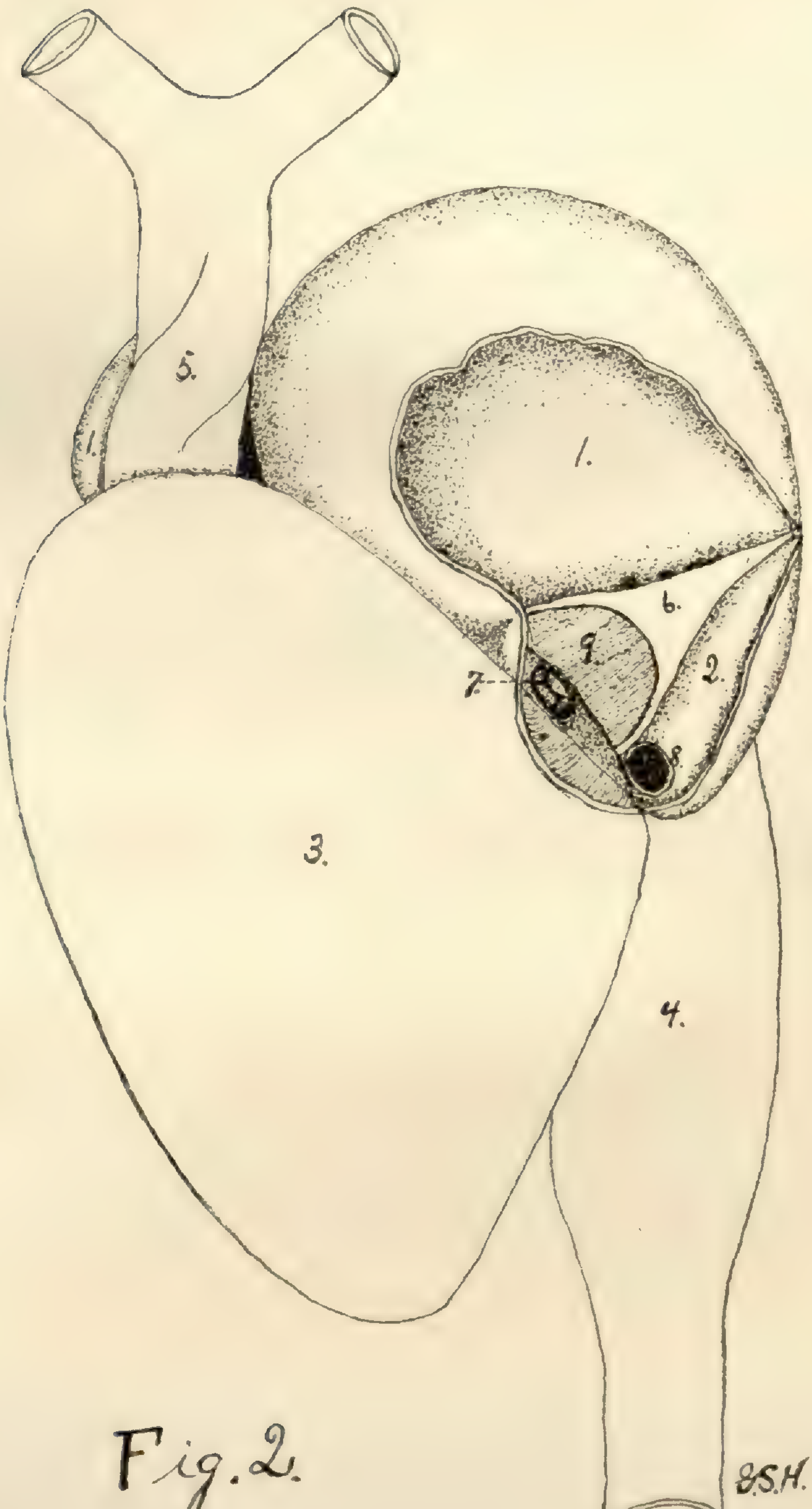


Fig. 2.

Hopkins on Desmognathus.

PLATE XVI.

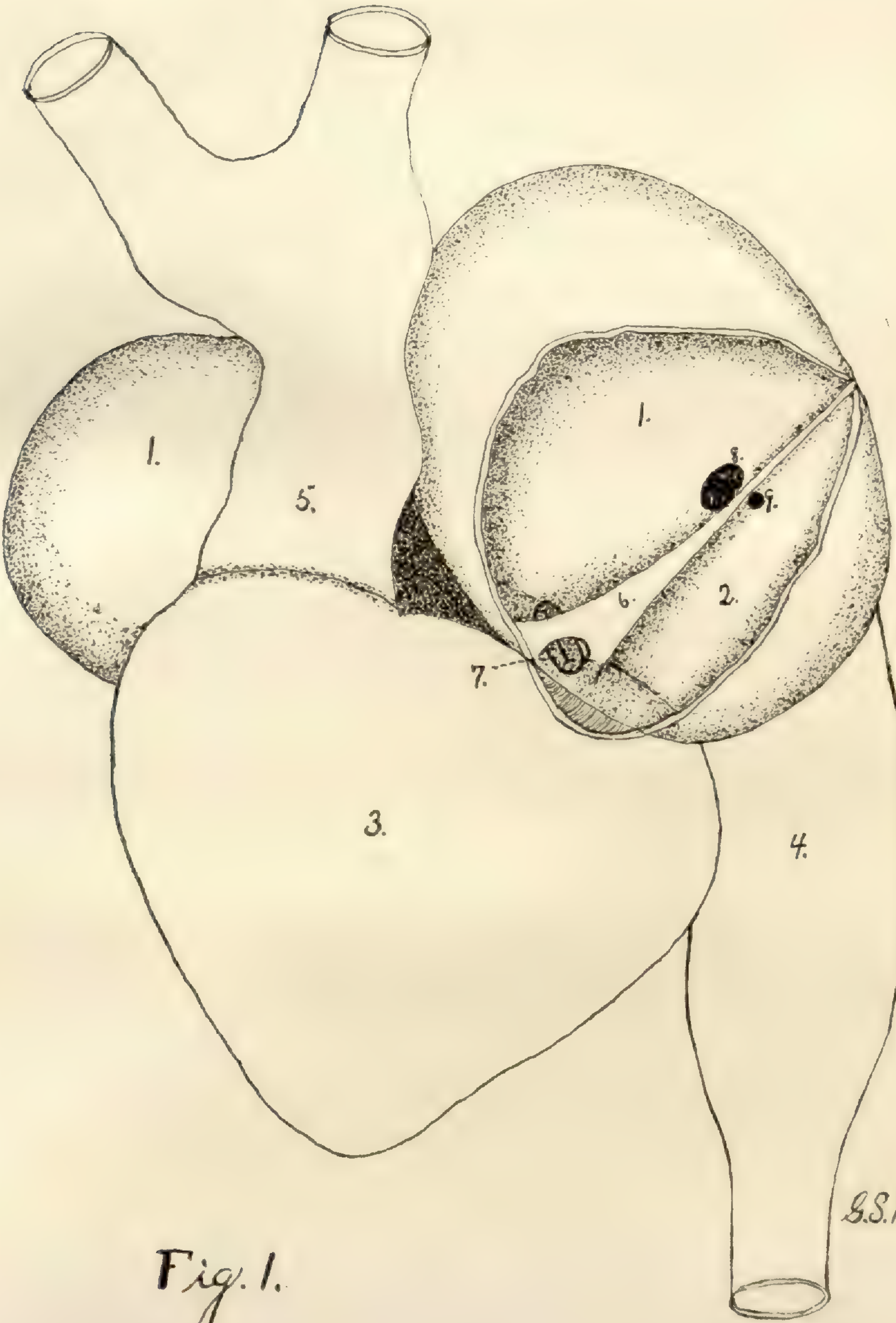


Fig. 1.

Hopkins on Diemyctylus.

forms that have been mentioned, as well as in other members of the class Batrachia, the sinus venosus opens distinctly into the right auricle and the pulmonary vein into the left.

Let us now compare the heart of a lungless salamander (Fig. 2.) with the one just described. The four parts, auricles, ventricle, bulbus arteriosus and sinus venosus are clearly recognizable and, superficially examined, present nothing unusual; it is only when the cavities are opened that the differences between the two hearts become apparent. One of the first things to attract attention is the left auricle. In the lungless forms examined, it is much smaller in comparison to the right than in Diemyctylus, for example, and *no pulmonary vein was found opening into it.*

The auricular septum has only one opening through it, or perhaps, more correctly, it extends only part way across the cavity, but this aperture in the septum is so large (Fig. 2, 9.) that it is believed the communication between the two cavities is more free than even in Necturus. Just what function or functions the septum may have in these lungless forms, it seems to me, is not quite clear. That it has but little, if any use, is indicated by the way the sinus venosus opens into the auricles. In place of opening into the right auricle only, as in the forms having lungs, *it opens more freely into the left auricle than into the right.* If the ventral parietes of the heart be removed, one can look directly into the opening of the sinus venosus from either of the auricles, but more directly into it from the left than from the right, for when seen from the latter, one must look through the large opening of the auricular septum, Fig. 2, 9. In salamanders with lungs, each auricle opens in common into the ventricle with about equal freedom of communication, whereas in the lungless forms the right auricle is in more direct communication with the ventricle than is the left.

Judging from the above facts, i. e., the way the sinus venosus opens into the auricles, the freedom with which the auricles communicate with each other, and the way the auricles communicate with the ventricle, it would seem as if the heart of the lungless salamanders, functionally, was only bilocular in place of being trilocular as in the rest of the Amphibia. Morphologically, of course, it is trilocular, but whether it is so physiologically seems to me doubtful.

The heart of 8 lungless species have been examined by the writer, and so far as was made out, all of them agree closely with the description as given above. The probabilities are that in all the lungless forms similar conditions of the heart will be found. Up to the present time 17 species and sub-species, either wholly without lungs or with only

functionless rudiments of them, have been reported. In his last paper, in which are enumerated 15 of the 17 lungless species, Wilder says that "in the Salamandridæ lungless species are as numerous as those possessing lungs, and that in consequence of this, the definition of the group must be modified." It seems, however, that even with his proposed additions, the definition is still not sufficiently comprehensive, for the peculiarities in the structure of the heart certainly have almost as profound a significance as the absence of the lungs themselves, and should be incorporated in any definition that may be given. In addition to the 17 lungless species already mentioned, the writer has found an additional one, *Spelerpes gluttolineatus.*

In order that one may see at a glance in which families and genera lungless individuals are found, the following table, taken from Prof. Cope's Batrachia of North America, is appended. [The last column is taken from the papers of Wilder and others].

Families.	Genera.	No. of species.	No. species without lungs or with only rudiments of them.
Cryptobranchidae	Cryptobranchus	2	
	Megalobatrachus	1	
Amblystomidae	Amblystoma	12 [N. A.] 1 [Siam]	1 A. opacum
	Chondrotus	7	
	Linguaelapsus	2	
	Dicamptodon	1	
Hynobiidae [all Asiatic]	Hynobius	5	
	Salamandrella	2	
	Onychodactylus	1	
	Ranidens	3	
	Batrachyperus	1	
Plethodontidae	Plethodon	8	2 P. cinerus, P. erythronotus, P. glutinosus
	Hemidactylium	1	
	Batrachoseps	4	1 B. attenuatus
	Stereochilus	1	
	Autodax	3	1 A. lugubris
	Geotriton	? [European]	1 G. fuscus
	Gyrinophilus	1	1 G. porphyriticus
	Manculus	2	1 M. quadridigitatus
	Spelerpes	9	3 S. bilineatus, S. ruber, S. gluttolineatus
	Oedipina		
	Oedipus	9	
Thoriidae	Thorius	1	1 O. variegatus
Desmognathidae	Desmognathus	3	1 D. fusca, D. f. brimleyorum, D. f. auriculatus

Families.	Genera.	No. of species.	No. species without lungs or with only rudiments of them.
Salamandridae [Old World]	Chioglossa	1	
	Salamandra	8	
	Hemisalamandra	1	
	Triturus	6	
	Pachytriton	1	
Pleurodelidae [All found in Old World; three species in N. A.]	Salamandrina	1	1 S. perspicillata
	Diemyctylus	10 [2 N. A. species]	
	Pleurodeles	1	
	Glossolega	3	
Amphiumidae	Amphiuma	1	
Coeciliidae	(numerous)	(numerous)	

In the last column of the above table, the figures indicate the number of species in which lungless individuals have been found. Where there is a discrepancy in the numerals and the number of species following them, it indicates either sub-species or species not mentioned in Cope's Batrachia of North America.

Description of Figures.

Fig. 1. Heart of Diemyctylus viridescens (semi-diagramatic) to show the general relations of the heart of a salamander with lungs. The ventral wall of the heart has been removed in order to show the auricular septum, the openings of the sinus venosus and the pulmonary vein, and also the relation of the auriculo-ventricular aperture to the right and left auricle.

1. Right auricle; 2. Left auricle; 3. Ventricle; 4. Sinus venosus; 5. Bulbus arteriosus; 6. Auricular septum; 7. Auriculo-ventricular aperture; 8. Aperture of sinus venosus; 9. Pulmonary vein.

Fig. 2. Heart of Desmognathus fusca (semi-diagramatic) to show relations of the heart in a lungless salamander. The ventral wall of the heart has been removed.

1. Right auricle; 2. Left auricle; 3. Ventricle; 4. Sinus venosus; 5. Bulbus arteriosus; 6. Auricular septum; 7. Auriculo-ventricular aperture; 8. Aperture of sinus venosus; 9. Opening through auricular septum.

G. S. Hopkins, D. Sc., Cornell Univ.

On two new Species of Lizards from Southern California.—Anota calidiarum Cope.—A single lateral fringe of conic scales, extending on three-quarters the length between the axilla and groin;

no trace of inferior fringe. Enlarged lateral gular scales only traceable below the rictus oris. Occipital horns moderate, each with a short accessory horn at the external base; internal temporal horn half as long as the occipital, with a short accessory horn at the external base; external temporal horn very short, and the temporal anterior to it presenting a serrate edge only. Infralabials presenting a serrate edge only; parietal region bounded on each side by an angulated border which overhangs the temporal region.

Squamation of the head smaller than in other species; superior labiáls twelve, below the middle of the eye, instead of eight or nine in *A. platyrhina* the nearest allied species. Seven subequal scales in the transverse row between the canthal rows on the frontal angle; there are five unequal scales on the corresponding position in the *A. platyrhina*. Six longitudinal rows of supraocular scales, of which a group of five or six posterior to the middle are larger, but unequal. Supraorbital rows in contact, except at points, on the median line; last superciliary presenting a sharp angle; penultimate also presenting a prominent angle. Tomia of mouth only moderately serrate; a row of conic scales rising posteriorly on the side of the neck, and above its posterior end an inconspicuous rosette. A conspicuous rosette above the middle of the humerus.

This species is nearest to the *A. platyrhina* Gir. from which it differs in various respects. The general proportions of all the parts and the coloration are about as in that species, the difference chiefly appearing in the squamation and the horns. The scales of the head are much more subdivided, and the presence of accessory horns is unique in the genus. The simplicity of the lateral fringe is also characteristic, as is also the rudimental character of the rosette on the neck.

Anota calidiarum Cope.

Catal. No.	Ariz. No.	No. species	Locality.	Source.	Obs.
8444	434	1	Death Valley, Cal.	U. S. Agric. Dept.	Alcoholic.

I may add here that in my estimation the genus Anota Hallow. is valid. It includes the species *A. modesta* Gir., *A. goodei* Stjen., *A. platyrhina* Gir., *A. maccallii* Hallow., and the above described.

SCELOPORUS VANDENBURGIANUS Cope.—This is a small species with small scales and very dark colors. There is not much difference in

the sizes of the dorsal, lateral and ventral scales; forty-five rows may be counted between the occiput and a line connecting the groins, and twelve in a head length. Between the groin and axilla thirty-five scales may be counted to an axillary area of smaller and smooth scales. The dorsal and lateral scales are keeled and mucronate; those of the inferior surfaces smooth and mostly feebly notched. Caudal scales strongly keeled and mucronate and larger than dorsals. Two parietals on each side, the anterior the larger, and extending to the narrow marginal supraocular row, so that there is only one frontoparietal on each side. A third parietal external to the other two. The frontal is not longitudinally divided. There is one series of six large supraoculars, separated from the frontals and frontoparietals all round by a series of small scales. External to the large supraoculars is a series of four much smaller polygonal flat scales much as in *S. biseriatus.* Between these and the superciliaries one row of still smaller scales (with an extra scale or two); two scales on the canthus rostralis. Head scales all smooth; six large free auricular scales. A single vertical prehumeral fold enclosing a pocket of granular scales; temporal scales keeled. The extended hind leg reaches to the auricular meatus. Temporal pores sixteen; male with postanal plates.

Color of adult male dark green above, with faint traces of a paler stripe on each side of the back, and of a few darker spots on each side of the middle line. Inferior surface dark blue, with a pale line in the middle of the abdomen. Femur spotted with blue below; tibia and tail light greenish below.

Measurements.

	MM.
Total length,	127
Length to vent,	57
Length to line of axilla,	22
Length to line of interparietal plate,	13
Length of hind leg,	38
Length of hind foot,	18
Length of fore leg,	23
Length of fore foot,	10

Sceloporus vandenburgianus Cope.

Catal. No.	No. spec.	Locality.	Donor.	Obs.
21931	1	Summit of Coast Range, San Diego Co., Cal.	Dr. E. A. Mearns.	Alcoholic.

I have seen of this species only one specimen, which is an adult male. The colors of the female may be expected to be somewhat lighter. I have dedicated it to Mr. Jno. Van Denburgh, of San Francisco, an able writer on herpetological subjects.—E. D. COPE.

Modification of the Brain during Growth.[2]—1. One of the most marked changes of the embryo brain is the formation of the great bend near its middle, giving rise to the cephalic fexure. The cause of the bend is unknown. After it is formed, the early appearance of optic fibers and those of the postcommissure and supracommissure tends to produce a comparatively fixed portion at this bend as shown by measurements of parts of the brain in soft shelled turtle and the cat. The later developing parts produce secondary changes in the form of the brain tube as the embryo takes on the specific characters of the adult.

2. The pons bend of embryo mammals is a feature of the brain of many non-mammalian embryos which do not possess a pons, and in specimens examined is associated with and seems to be due to the early and enormous development of the Gasserian ganglion and its union with the brain by the fifth nerve. Later these parts are overshadowed by the growth of other parts and the pons bend becomes obscure.

3. In the cat the pons bend is exaggerated by the formation of a great fist shaped mass of cells at the surface on each side of the meson. This mass of cells is continuous with a conspicuous layer of cells which extends upon the surface to the union of the solid wall of the oblongata with the membranous roof or tela. This layer and cell mass seem to be proliferations such as His and Herrick have described as occurring at the union of a solid wall with a membrane. Later these cells are covered by pons fibers.

4. In the soft shelled turtle and the cat, early stages show clearly that the prosotela, the membrane included by the edges of the rima in the cerebrum, is a continuation of the membranous roof of the diencephal, such that if the brain were plastic the continuation of each side could be brought to occupy the dorsal surface of its cerebrum. A condition would thus occur which is comparable with the actual condition found by Wilder in Ceratodus. It may be of value in the further determination of homologies between the brain of fishes and mammals, as in fishes the membranous roof of cerebrum and diencephal is continuous.

[2] Abstract of paper read before the Amer. Asso. Adv. Sci., Aug. 24, 1896.

5. The dorsal (sensory) and ventral (motor) zones of His, demarcated by a sulcus extending from the myel through the brain to the optic recess, have not been verified in the forms examined (cat, turtle, amphibia, bird). The indications are rather that an original segmented condition is partially disguised by a secondary formation of sulci which extend in a cephalo-caudal direction. None of the five such sulci found in the oblongata could be said to separate the sensory from the motor region. The dorsal and ventral of them demarcate raphés. One of them occurs *in* the sensory, two *in* the motor, but there is no dividing sulcus between the two regions. The extreme point to which any of them could be traced was in the region of the albicans, none of them reaching the porta or the optic recess. A second group of sulci arising in the optic and preoptic recesses extend to the porta, two of them passing through the porta to form the boundary of the striatum.

SUSANNA PHELPS GAGE.

The Lion of India.—The report that *Uncia leo* is found at the present time in the environs of Guzerate and Kutch appears to be an error. It has probably never existed in the latter locality, and is now to be met with only in the forest of Gir in the Kathiawar. It has disappeared from Rajkot, where it was abundant in 1832, from the hills of Bardo, and from many other localities where it formerly existed in great numbers, nor has it been seen for a long time in the forest above mentioned. Formerly hunters very seldom ventured in that region for fear of the bandits who were in the habit of taking refuge there, and also for fear of contracting fever. Gradually, however, the forest is being cleared away, settlements are being made, and the domain of the lion is being curtailed. To prevent total extinction of the species, the Durbar of Kathiawar has forbidden lion hunts for a period of six years. But this will do no good unless at the same time a forest reservation is made.

The popular belief that this species is without a mane in India is another error that is corrected. (Revue Scientif. Août, 1896).

Inheritance of Artificial Mutilations.—The instance cited by Mr. Norman E. Hills (in the September Naturalist) of the birth of short-tailed fox terriors, is striking in showing a larger proportion of deformed puppies than is common in such cases, but instances like the one cited are frequently noted in the press devoted to dogs, and concerning several breeds that have been mutilated for generations.

But to thoroughly consider the matter of the inheritance of artificial deformities, the cases of breeds in which the deformity is usually

natural, should be considered. I believe the tailless Manx cats generally come in that shape, and this I know is often the case with the bobtail sheep-dog of England, and this is stated of several breeds of dogs, generally of the type of the bobtail sheep-dog found in other countries, Norway, Southern Russia and elsewhere.

But the peculiar feature of this inheritance is its freakishness. Two naturally long-tailed parents have produced a tailless dog, in whom the potency was so strong that no bitch, no matter what the breed was, ever produced a full, natural tailed puppy to him. I remember of one puppy by this dog, ex his double grandam (he was the son of litter brother and sister) whose tail was of usual length, but had a deformity in it as though it were tied in a knot. Again, it is not at all uncommon for a bitch to begin production with all naturally long-tailed puppies, and after some years, change to always producing some tailless ones, even when mated with mongrel dogs, while some bitches reverse the order, beginning with tailless ones and changing to full-tailed ones. I recently noted a reported instance of just such a change of production in a Manx cat, and it seems to me that this freakishness introduces a very disturbing element into consideration of the question of inheritance.

As an allied matter, please permit me to say that the notion that if a bitch has a mongrel litter she will thereafter *always* produce puppies showing traces of the unallied sire, is rank rubbish, and on the point that this occurrence is *not* invariable, any experienced breeder will concur, as very, very few such breeders have ever seen such a case. For myself, I have bred dogs for over forty years, have bred many mongrel litters, and never saw a case of telegony—or, as we commonly call it, "influence of previous sire." That this influence does sometimes show itself, is beyond doubt; but some very extended inquiries of mine some years since, showed that it was shown only in about one per cent. of cases of mesaliances; and when it was considered that an instance of this "influence" will be remembered from its extraordinary character, while instances where it does not occur are forgotten, being the expected result, I believe that the one-tenth of one per cent. of cases will be found to be the extent of its occurrence. It is very strange that those scientific men who uphold the idea of the invariable occurrence of this "influence," all overlook the potent fact that the "influence" shows itself invariably only in the skin and hair, never affecting conformation. In view of this, the theory propounded by Dr. Jonathan Hutchinson, President Royal College of Surgeons; Dr. J. Sidney Turner, President South British Medical Society, and Everett Millais, Esq., seems sound, and bears against the idea of the bitch being herself

bastardized, that theory being that unripe ova are partially impregnated by the spermatozoa of the foreign male, not sufficiently to fully vivify them, the influence of this impregnation affecting only the epiblast, from which the skin is evolved, and a subsequent fertilization brings full life to the ova, determining other features of the fœtus. Thus, in the case of a pug bitch, which had a mongrel litter by a Skye terrior, and at her subsequent whelping by a pug dog, had some puppies with rough hair, these "influenced" puppies had the conformation of the pug all over, even to the twisted tail.

However, be the scientific part of the question what it may, the too common idea that a bitch having a mongrel litter will show influence of that litter in all future offspring, is utterly fallacious.

Yours truly, W. Wade.

Oakmont, Pa., Sept. 14, 1896.

ENTOMOLOGY.[1]

A New Character in the Colobognatha, with Drawings of Siphonotus.—In all known Diplopoda the external seminal apertures are located just behind the second pair of legs or in the coxæ of the second legs. In all Diplopoda except Polyxenus one or more pairs of legs are more or less modified to assist in copulation. In a great majority of forms the legs most modified are those of the seventh segment, but in two groups, the Omniscomorpha and Limacomorpha of Pocock the legs of the seventh segment are unmodified, while one or more pairs at the caudal extremity of the body are transformed into copulatory organs. The modification which has taken place in the Limacomorpha is very slight, for according to Mr. Pocock's descriptions and figures the last legs of *Glomeridesmus marmoreus*[2] consist of four or five joints and differ from the others mostly in being shortened and thickened. An equal or greater degree of specialization is now known to exist in other legs than those of the seventh segment, indeed, an almost equal peculiarity of structure is sometimes manifested by nearly all the male legs of certain genera of Polydesmidæ, Spirostreptidæ and Spirobolidæ. Especially noteworthy are the first pair of legs in Iulidæ, Parajulidæ and Pæromopodidæ; the second pair of legs of Stemmatoiulidæ and Parajulidæ; the third pair in Striariidæ. With-

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

[2] Journ. Linn. Soc. Zool., XXIV, 475.

out detailing the very numerous and striking contrivances displayed by nearly all the anterior legs of some Craspedosomatidæ, it is sufficient to point out that in Scytonotus[3] modifications apparently as great as those of Glomeridesmus occur as far back as the twentieth pair of legs. In the light of these facts the degree of modification shown by Glomeridesmus counts for little or nothing as an evidence of relationship with the Oniscomorpha. It might be said that Glomeridesmus has no copulatory legs at all, for the structures figured by Mr. Pocock are probably not comparable with the true copulatory legs of the other Diplopod groups, either in structure or function. The really remarkable thing about Glomeridesmus is that the legs of the seventh segment are not modified. Yet on this account we are not obliged to arrange Glomeridesmus in a separate category, for the degrees of modification to be found in the legs of the seventh segment of the other Diplopod groups are very various. It is even possible to trace, in the second pair of legs of the seventh segment of Craspedosomatidæ all the stages from the nearly normal to the completely modified condition. Thus with reference to the fact that the seventh legs are unmodified, Glomeridesmus may be looked upon as one end of a series, not necessarily farther removed from the other groups than they are from each other. Certainly the distance between the unmodified legs of Glomeridesmus and the distinctly jointed copulatory legs of Polydesmoidea and Polyzonoidea is not greater than that between those of the Polydesmoidea and the Spirostreptoidea.

If, however, we admit that differences in the position and degree of modification of legs transformed for copulatory purposes are not of themselves characters of fundamental importance in the Diplopoda, we may seem to be under the necessity of admitting in addition that the constant appearance of what we may call the true copulatory legs in the place of the anterior or both pairs of the seventh segment is an evidence that the Helminthomorpha of Pocock are a homogeneous group to the extent of having more affinity with each other than with the Oniscomorpha. If, however, facts exist which indicate that the copulatory legs may have had independent origins in any of the Helminthomorpha the evidence just referred to is largely overthrown, for the utter diversity in plan of the copulatory legs of the different orders of Diplopoda is itself a strong indication that they represent independent lines of development. Such seems to be the import of the fact that the legs which in the Merocheta[4] are primarily transformed

[3] Am. N. Y. Acad. Sci., VIII, 233 (1894).

[4] An ordinal name to cover the Polydesmoidea, Craspedosomatoidea and Callipodoidea. Cf. Ann. N. Y. Acad. Sci., Vol. IX, (1895).

into copulatory organs, the anterior pair of the seventh segment, are in the Colobognatha *entirely unmodified*. Hence we must suppose that either the legs or the function have migrated, in case we assume a common origin and attempt to homologize the copulatory legs in the two orders. The theory of migration, however, has no facts to support it, and would be equally fatal to the idea that affinity or the want of it can in the Diplopoda be inferred from the position of the copulatory legs.

The fact that the Colobognatha have eight precopulatory legs is not new, but up to this time the whole eight have been supposed to belong to the first six segments. Both Latzel and Pocock[5] give the distribution of these legs as in the second column. In reality the arrangement is that of the third column.

	Latzel.	*Siphonotus.*
First segment,	First pair,	First pair.
Second segment,	Second pair,	Second pair.
Third segment,	Footless,	Third pair.
Fourth segment,	Third pair, Fourth pair.	Fourth pair.
Fifth segment,	Fifth pair, Sixth pair.	Fifth pair.
Sixth segment,	Seventh pair, Eighth pair,	Sixth pair. Seventh pair.
Seventh segment,	First copulatory, Second copulatory,	Eighth pair. First copulatory.
Eighth segment,	Eleventh pair. Twelfth pair,	Second copulatory. Eleventh pair.

My attention was first attracted to these facts while engaged in examining specimens of Siphonotus collected in Sierra Leone in December, 1893. The creatures were abundant in decaying banana stumps in Freetow, and I secured a large quantity. Instead of curling up as nearly all the representatives the present order are accustomed to do when placed in alcohol, my specimens remained conveniently straight and pliable so that they could be mounted in alcohol or balsam and studied to advantage. Of the arrangement of the legs as here stated there can be no doubt. The drawings are mostly camera tracings made from preparations in balsam. In order to make sure of the condition in Polyzonium, the genus studied by Latzel, I cut animals in two horizontally, brushed away the internal structures and mounted in balsam.

[5] Mr. Pocock seems to have come to doubt this disposition, for he uses a "?" in front of his last statement on the subject.—Max Weber's Reise, p. 335.

Without such a preparation satisfactory observation is very difficult if not impossible in Polyzonium, for all the parts, especially the bases of the legs, are crowded together. I have examined in addition Andrognathus, Platydesmus, Pseudodesmus, Siphonorhinus and Siphonophora without finding any indications that the condition described is not present in all, though a final demonstration would in most cases not be easy without dissection.

Probably correlated with the comparatively slight degree of specialization which appears in the copulatory legs of the Colobognatha is the fact that in young males of Siphonotus the copulatory legs are several-jointed before maturity. Such a condition seems to be unknown in the other helminthomorphous groups.

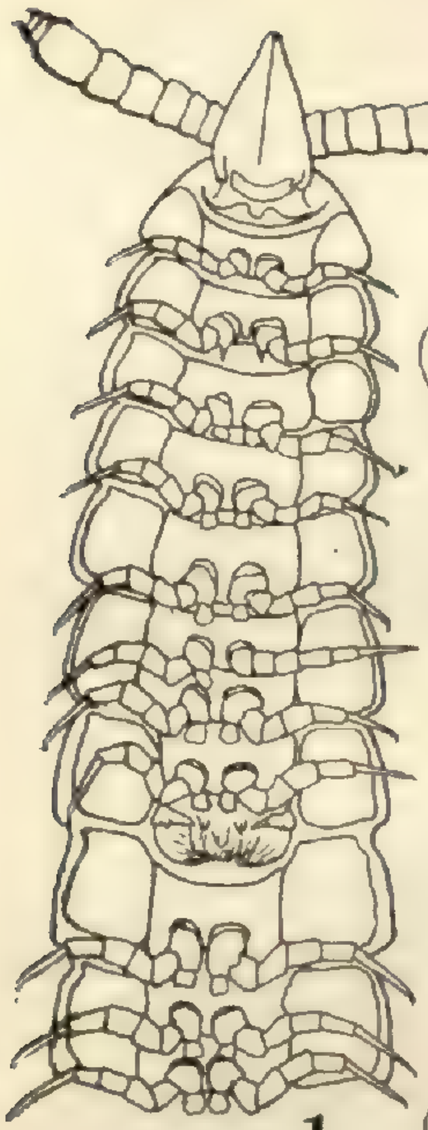
In the previous discussion there has been no intention to imply that the orders Oniscomorpha and Limacomorpha are not valid; the contention is merely that the position of the modified legs does not *of itself* justify holding them as divisions of greater weight than other natural groups of Diplopoda, some of which have been designated by ordinal names. It is to be expected that future study may result in a natural arrangement of the groups now designated as orders, but until their affinities are demonstrated nothing is to be gained by attempting to retain under one ordinal name and description animals which may prove to be widely divergent in their development history. Thus it is by no means impossible that the Colobognatha are really a group farther removed from the other Helminthomorpha than are some of these latter from the Oniscomorpha. Many of the peculiar characters of the Oniscomorpha are evidently the result of their power to roll themselves into a sphere, and are not to be assigned great weight in estimating affinities.

Systematic Note.—The genus Siphonotus has not until very recently been reported since its establishment by Brandt in 1836. Within the last year or two Mr. Pocock has described species from St. Vincent (West Indies), Java and Celebes. To me it seems doubtful whether any of these species are congeneric with Brandt's type *S. brasiliensis*. Provisionally, however, the species of which drawings are presented may be described under Siphonotus, no doubt being possible that its affinities are here rather than with any other genus yet established.

Siphonotus africanus sp. n.

Body slender, the sides parallel to near the ends, or very slightly converging cephalad.

PLATE XVIII.



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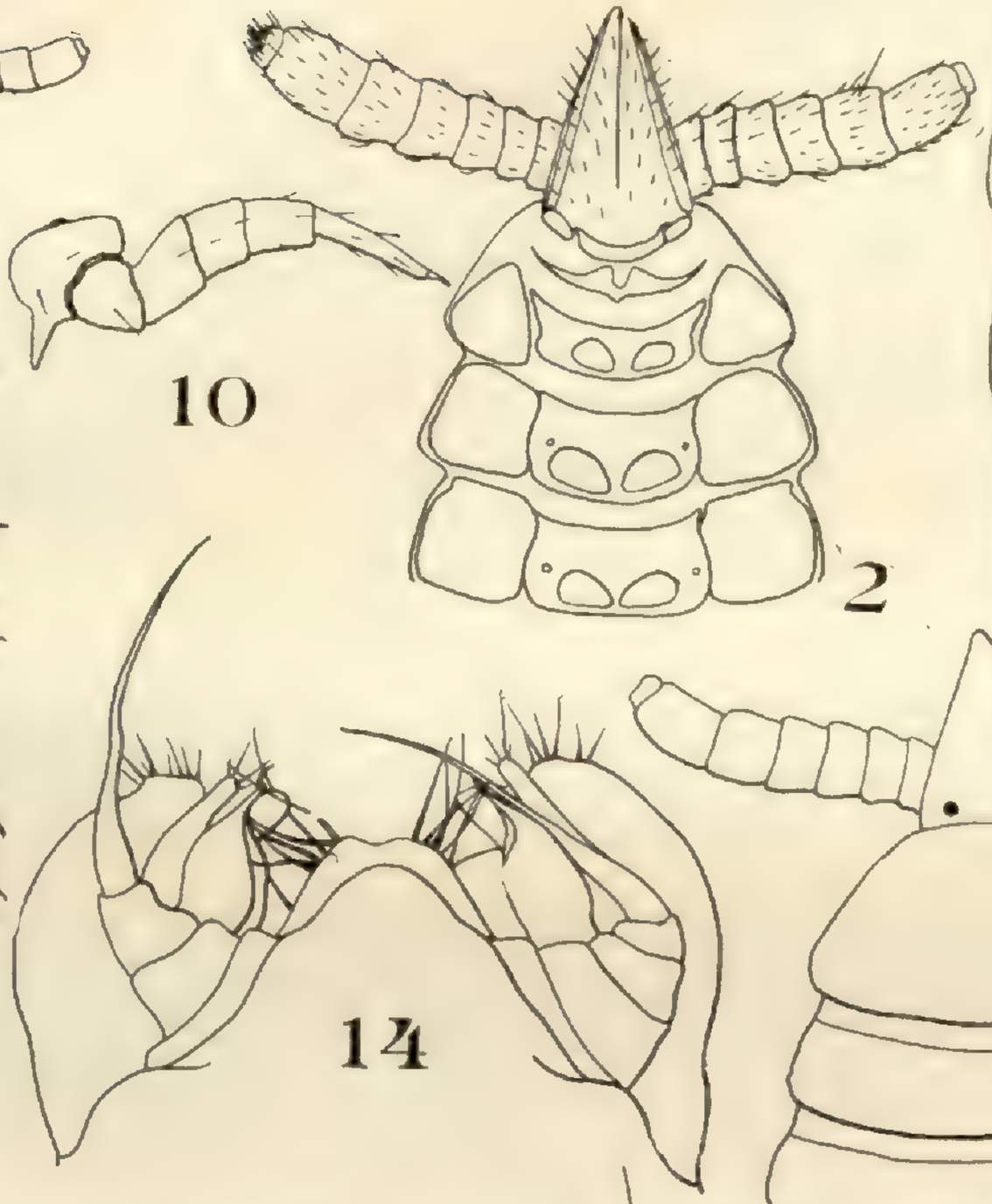

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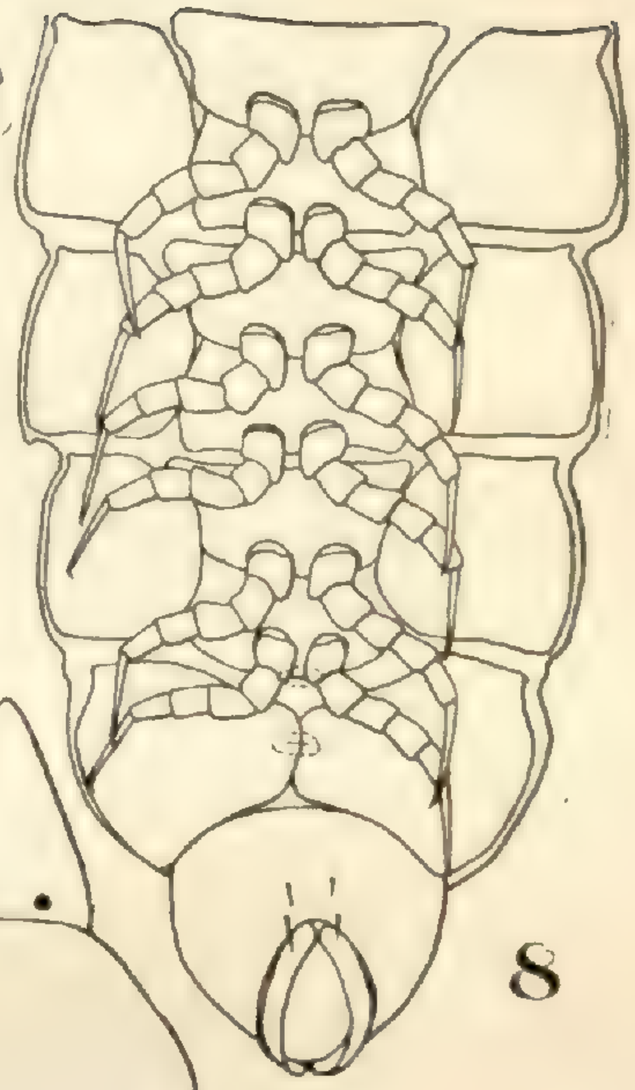

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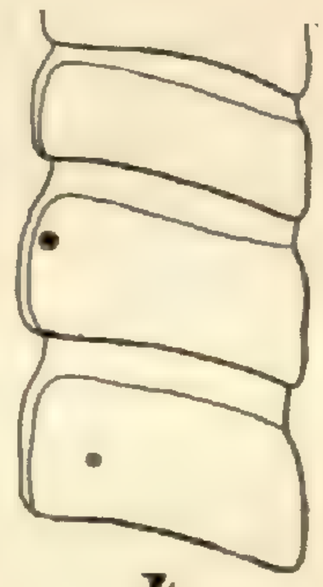

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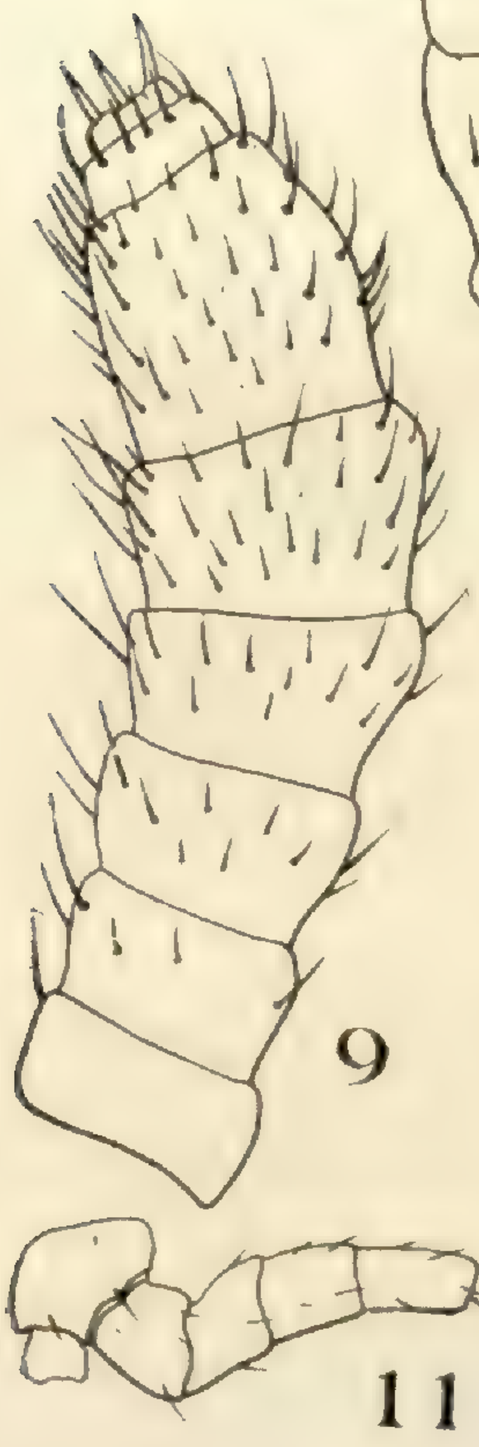

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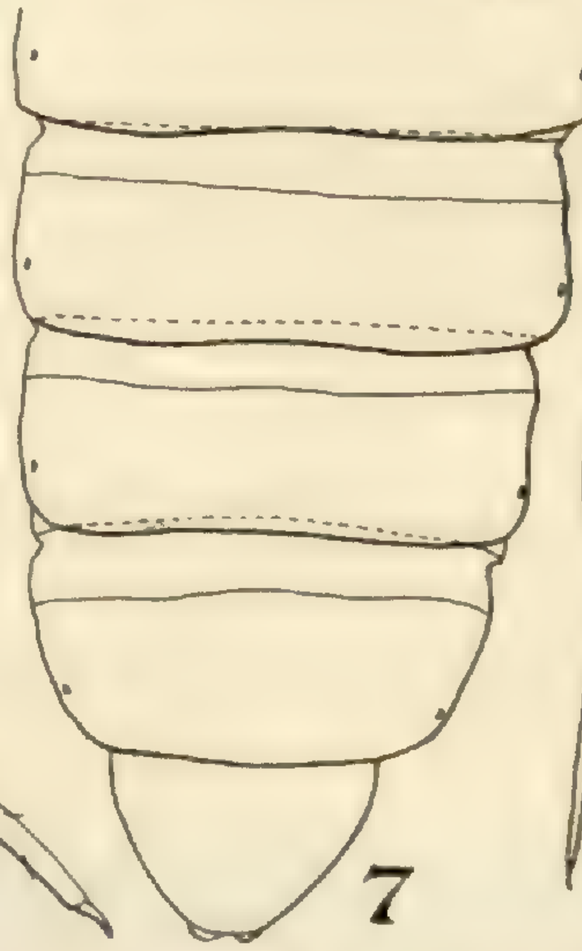

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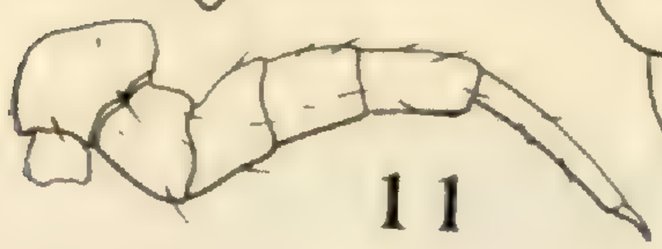

11

Siphonotus africanus Cook.

Head smooth and shining, sparsely hirsute distad.

Eyes of a single ocellus on each side, large and strongly pigmented.

Antennæ sparsely hirsute, strongly crassate, scarcely clavate; joints increasing in length from the first to the sixth.

Hypostoma distinct, medianly deeply excavate.

Mentum distinct, on each side a large cardo (?). Other parts of gnathochilarium indistinct; the lines of the figures may represent internal structures only.

First segment longer than the others, which increase in length to the middle of the body and are scarcely shorter caudad; surface of all the segments smooth and shining.

Repugnatorial pores in a continuous series from the fifth segment, removed considerably from the margin of the scuta, except those of the fifth segment, which are also distinctly larger than the others.

Pleuræ large and entirely free, smooth and shining.

Pedigerous laminæ distinct, free; sometimes the edges of the pediger. ous laminæ and the pleuræ lie one upon the other.

Penultimate segment without legs, in contact or overlapping, but not closed below.

Anal valves narrow; preanal scale wanting.

Legs sparsely hirsute, all six-jointed, with a rudimentary second joint which would make seven.

Males with a conic process from the coxæ of the second legs. All the other legs except the first have a coxal aperture from which projects a transparent membrane or secretion.

Male genitalia, see figures.

Color of living animals, pinkish, pale to dull reddish-pink.

Length of mature individuals up to 15 mm., width .75 mm.; segments of adults 39–55. The males are distinctly smaller than the females. The young male specimen from which fig. 15 was drawn has 24 segments, all but two of which bear legs, as in the mature animals.—O. F. Cook.

Explanation of Plate.

Siphonotus africanus.

Fig. 1. Head and first nine segments, ventral view, to show arrangement of legs.

Fig. 2. Head and first three segments, ventral view, the legs removed to show pedigerous laminæ, spiracles, etc.

Fig. 3. Head and five segments, dorsal view.

Fig. 4. Segments 4–6, lateral view, to show the peculiarly enlarged pore of the fifth segment.

Fig. 5. Three normal segments, dorsal view, less magnified than fig. 3.
Fig. 6. Diagrammatic cross-section, showing relative positions of the parts of the exo-skeleton.
Fig. 7. Last five segments, dorsal view.
Fig. 8. Same, ventral view.
Fig. 9. Antenna, much magnified, ventral view.
Fig. 10. Second leg of male.
Fig. 11. Eleventh leg of male.
Fig. 12. Last leg of male.
Fig. 13. Male copulatory legs, dorsal view.
Fig. 14. Same, ventral view.
Fig. 15. Segments seven and eight of young male.—O. F. Cook.

PSYCHOLOGY.[1]

Congress of Psychologists.—The third Congress of Psychologists was held at Munich, August 4th to 9th, Prof. Stumpf, of Berlin, presiding. It was the largest and in many respects the most successful of the three. Of course the German attendance was fuller than at the last one, held in London in 1892, and German delegates are always most welcome. When we take into account the fact that Germany is to-day the country where psychology is most vigorously and successfully pursued, it follows that this Congress was, up to date, the greatest gathering of eminent psychologists ever seen. As to France, the attendance was disappointing in numbers, although the delegation was very representative; and the same is true of the British contingent. The other countries, except America, were adequately represented; the small attendance from our side of the water being a matter of the more surprise in view of the tendency of our professors to take their vacations abroad—indeed, the attendance at the last Congress in London was considerably larger.

In its general character, the tendency to allow the popular attendance upon the meetings to swamp the scientific proceedings was more marked in Munich, and it is not too much to say that this constituted a very great defect in the arrangements. The membership was over four hundred. There was a constant flow from hall to hall, and the corridors

[1] Edited by H. C. Warren, Princeton University, Princeton, N. J.

were filled with bewildered persons. Some limit must be put on the popular membership at the next congress, or the scientific people will yield the field to the sightseers and amateurs. The other possible improvement comes to the front again apropos of this meeting in Munich —the crowded condition of the programme. Besides the general meetings, which came in the forenoon sessions, the committee arranged for five sections, all running simultaneously and all subject to constant give and take, as respects their audiences, from one to another. Besides the constant interruptions and great confusion which this produced, it practically prevented a person from hearing many readers whom he especially desired to hear. Since the time limits were not enforced upon the papers or discussions, one could never tell how far on this section or that had progressed, and so could not time his presence for any particular reader. Moreover, the papers were as usual so generally accepted by the committee—anyone who wanted to present something had only to send his name and topic beforehand—that many were read which were of little or no scientific value; and the titles of papers were entered on the programme in advance, so that there was no way to learn infallibly whether a particular reader had arrived and would present his dissertation or not. The gaps left by the absentees were consequently quite an unknown quantity. Every such meeting should have a committee to read and select from available papers, arrange them strictly according to unchangeable time divisions, and require each reader to report finally a day or two before the meeting as to his actual attendance, the final programme being only then printed. This would have the further advantage of ruling out titles and names which are from the first doubtful; for it is astonishing to what an extent men fail to carry out what should be their serious intention when they give their names to be printed on these Congress programmes.

So much for the general character of the Congress. Of course, this is not the place for an account, in any detail, of its scientific features. The division into sections will show something of the remarkable range that modern psychology finds itself obliged to take: "Normal," "Sleep and Hypnotism," "Mental Pathology," "Neurology," "The Senses and Psychophysics"—the titles being somewhat abbreviated in this list. In each of the sections there were some great papers and one or more lively discussions. The most interesting thing in the way of neurological work—it was presented, however, in one of the general meetings—was the paper by the veteran Flechsig on "Association Centres." It will be remembered that Prof. Flechsig has been engaged for some time on comparative studies of the brains of human infants at different ages,

attempting so to arrive at a view of the order of development of the elementary mental functions, with the corresponding progress in brain anatomy and physiology. He has published very rich results from time to time, and among them is his determination of certain so-called "association centres." He thinks that the much discussed frontal region of the brain is the location of associations of a higher and more abstract kind; and that in the region back of the well-known "motor region," extending to the visual centre in the occipital region, is a great centre for the associations which bind the sense functions together. This in brief, and without the discriminations which an accurate account of his views should make. The reason which he gives for these determinations is that only after some growth, and after the senses are well developed, do we find the great masses of connecting fibres which traverse these regions forming in the child's brain.

Apart from the question of fact, as to which Prof. Flechsig's researches may be considered as being of the greatest importance (especially when we consider his method), it is difficult to see how these regions can be, in any true sense, "association centres;" for, admitting that the connections between the sense centres run through these regions, the main thing about the associations must be the things associated, not the mere fact of connections between them. One would hardly call the bunch of telegraph wires on the housetop a "communication centre;" the loci of communication are still at the telegraph offices. Without them, the wires would be possibly even more helpless than the offices without the wires. Prof. Flechsig's paper was a model for imitation in the manner of its presentation, and its interest was enhanced by slides showing the infant's brain, in sections illustrating the periods of its growth.

Another contribution to the understanding of the relation of psychology and brain physiology was that of the well known neurologist, Prof. Edinger, of Frankfort, on the question, "Can Psychology make use of the results so far attained in Brain Anatomy?" He did not confine himself to anatomy, but presented a series of interesting notices on the development of the nervous system in the scale of life, and made a strong plea for a corresponding genetic study of comparative psychology. Genetic psychology, he says, is so far behind analytic psychology because psychologists have confined their attention, on the anatomical side, to the cerebral hemispheres, while what they should do is to study the evolution of the nervous system all the way up, and see the progress of consciousness with it. "Gerade auf diesem Gebiete müssen anatomisch-physiologische und psychologische Studien durchaus Hand in Hand gehen." All this is true and remarkably opportune, I think,

despite the fact, that in his main illustration Prof. Edinger falls into one of the glaring fallacies into which this sort of analogy between body and mind may lead. He says there are certain creatures (fishes) which have no hemispheres, and it follows that, on the psychological side, we must deny to these creatures "all that the hemispheres are necessary for in the higher creatures." This overlooks the great principle that, in the lower forms, less differentiated structures may do what more differentiated ones do in the higher forms. To press this point consistently, he would seem to have to deny consciousness altogether to these fishes. The lesson of this paper, however, is a most timely one; psychologists, especially in Germany, are not half awake to the genetic problem, and when they do awake, no doubt it is true that the richest lessons that the physiology of the nervous system will have to teach them will be derived from such comparative study as Prof. Edinger advises.

Several papers of general interest were read in the open meetings. The President's address was rather more severe and *wissenschaftlich* than the earlier addresses of the presiding officers have been, but it was an exceedingly interesting and discriminating review of theories on the connection of mind and body. The arraignment of Parallelism was very effective—possibly more so than the positive doctrine of the paper. Prof. Ebbinghaus of Breslau gave a new way of testing the mental condition of school children at different periods and in different conditions of fatigue, etc. It differs from the methods already in vogue in that it endeavors to test the child's correlating or apperceptive faculty rather than his sense-perceptions or his memory. The method, which teachers will find extremely interesting, consists in taking a passage from some interesting narrative-text, and, after striking out various words and phrases and printing the passage with black spaces where these erasures have been made, telling the child to fill in the spaces as he thinks the sense requires. This requirement certainly calls upon the child for more than memory, and the results of its application, as reported by Prof. Ebbinghaus, seem to show its superiority; but it would appear to be applicable to children of a more advanced age, after the memory tests are outgrown. This general judgment, however, I must make with reservation, since the synopsis of the paper did not reach my hands. This may suffice to indicate the scope of the method, and to call the attention of our educational authorities to it. They will also be interested in Prof. Ebbinghaus's severe criticism of what he called the "American method" of testing the mental condition of school children by the memory tests.

The fact that the papers on "Hypnotism" were fewer than in earlier congresses, in proportion to the entire number, and that there were a bare half-dozen on thought-transference and telepathy, shows the general tendency of psychology. The hypnotic period is past, even in France. Not that the gain from the study of hypnotism has not been permanent and great; on the contrary, its results are only now getting so absorbed into the body of psychological truth that it no longer makes sensational appeals for a hearing. As to telepathy, I think there is a real decay of interest in the subject, much as this is to be deplored. The most interesting paper in the hypnotic field was a general one by Prof. Pierre Janet.

The section on the Senses and Psychophysics did much exact work. Dr. Stratton of the University of California communicated some valuable experiments of his own on the artificial reinverting of the retinal image and its effects on the sense of bodily position in space, which will be of especial interest to those who think the normal inversion of the image requires a theory.

Two other general questions of great interest were discussed, with as much ability as vehemence, by the Vice-President of the Congress, Prof. Lipps, of Munich. One of his papers was a very important contribution in the sadly neglected field of the æsthetics of visual form. I can do no more than recommend his paper in the Congress "Proceedings" (to appear very soon) to those who are concerned with elementary æsthetic principles. The other topic was the much-discussed one on the "Unconscious" in psychology. The question, Can mental states be unconscious? has a peculiar fascination, because of the great number of verbal distinctions of which it admits. It must be confessed that Prof. Lipps's paper did not make the number of these verbal distinctions less. He reaches a sort of return to the soul-substance theory—a hidden thing in which mental states, and especially tendencies of an active kind, may be preserved when we are not conscious of them. This has long ago been refuted as a general conception, I think; but the main point of interest, and that for which I bring the matter up, is that the results of pathology, dual consciousness, "multiple personality," etc., which are considered by many as giving the strongest evidence for the "unconscious," require quite a different theory. The "unconscious" of the pathologists is a body of conscious data gathered into a new and secondary consciousness of its own. While these states of mind are not conscious to the major person—and so, by a certain license, are called "unconscious"—still it is just the evidence that they are conscious in their own way and in their own seat in the nervous

system that enables us to say that they are mental. So all this evidence goes, after all, to show a correspondence between the mental and the conscious. This Prof. Lipps does not seem to see, and his treatment of the question from a purely verbal and analytic point of view was consequently very inadequate.

In the higher fields of ethics and anthropology there were interesting papers, of which my space allows the mention of only one on "Ethical Values," by Prof. Ehrenfels (just called from Vienna to Prague), and one on the "Category of Individuality in Savage Thought," by Mr. Stout, the editor of *Mind*. Mr. Stout, I may add, has just been called to a lectureship in comparative psychology in the University of Aberdeen—a novelty for the British Isles, but appropriate in the institution which Prof. Bain has made famous in connection with psychological study. The next Congress is to meet in Paris in 1900 in connection with the Universal Exposition. Prof. Ribot will be President; M. Ch. Richet, Vice-President, and M. Pierre Janet, Secretary. The International Committee for the Paris meeting has the following American representatives: Profs. James (Harvard), Titchener (Cornell), Hall (Clark), and Baldwin (Princeton).

I cannot close this letter without referring—with profound regret, which many other American students of philosophy must also feel—to the death Prof. Avenarius, of Zurich, on August 18. Where I now write, the feeling that one of the greatest philosophical thinkers of Europe no longer adorns a Swiss university is very acute; and those who know the work of Prof. Avenarius must feel it also, regardless of the place of their habitation.—J. Mark Baldwin, in *New York Evening Post*, Sept. 12.

Mental Action During Sleep, or Sub-Conscious Reasoning.—Shortly after reading the interesting article by Professor Cope with regard to recent ethnological discoveries in Assyria, undertaken under the auspices of the University of Pennsylvania, and elucidated by Professor Hilprecht, I met with the account of a peculiarly curious dream which had been experienced by Professor Hilprecht whilst his mind was deeply occupied with these very investigations.

It is of course well known to all students of mental psychology, that the most complicated, abstruse forms of reasoning have often been carried out in dreams; and many interesting and well authenticated cases of this phenomenon will be found in Dr. Carpenter's Mental Physiology.[2] But the peculiarity of Dr. Hilprecht's dream consists in

[2] Chap. XIII, Unconscious Cerebration. Mental Physiology. W. R. Carpenter, M. D. Chap. XV, Of Sleep, Dreaming and Somnambulism, pp. 534, 593–5.

the intensely dramatic manner in which the solution of the problem he was engaged on was conveyed to his mind. I will now simply quote from the account given to Prof. William Romaine Newbold, by Professor Hilprecht, in the first place of a train of sub-conscious reasoning during sleep which put him on the track of a satisfactory rendering of an Assyrian proper name; and in the second place of the work carried out under the influence of a strangely dramatic dream.[3]

"During the winter 1882–3, Professor Hilprecht was working with Professor Friedrich Delitzsch, and was preparing to publish as his dissertation, a text, transliteration and translation of a stone of Nebuchadnezzar I, with notes. He accepted at that time the explanation given by Professor Delitzsch of the name Nebuchadnezzar, 'Nabû-kudurru-usur,' 'Nebo protect my mason's pad' or mortar board,' *i. e.*, 'my work as a builder.' One night, after working late, he went to bed about two o'clock in the morning. After a somewhat restless sleep, he awoke with his mind full of the thought that the name should be translated 'Nebo protect my boundary.' He had a dim consciousness of having been working at his table in a dream, but could never recall the details of the process by which he arrived at this conclusion. Reflecting upon it when awake, however, he at once saw that *kudurru*, 'boundary,' could be derived from the verb *kadaru*, to enclose. Shortly afterwards he published this translation in his dissertation, and it has since been universally adopted."

Mr. Newbold then gives a translation from the account written in German by Prof. Hilprecht of his remarkable dream.

"One Saturday evening, about the middle of March, 1893, I had been wearying myself, as I had done so often in the weeks preceding, in the vain attempt to decipher two small fragments of agate which were supposed to belong to the finger rings of some Babylonian. The labor was much increased by the fact that the fragments presented remnants only of characters and lines, that dozens of similar small fragments had been found in the temple of Bel, at Nippur, with which nothing could be done, that in this case furthermore I had never had the originals before me, but only a hasty sketch made by one of the members of the expedition sent by the University of Pennsylvania to Babylonia. I could not say more than that the fragments, taking into consideration the place where they were found and the peculiar characteristics of the cuneiform characters preserved upon them, sprang from the Cassite period of Babylonian history (ca. 1700–1140 B. C.); moreover, as the first character of the third line of the first fragment seemed to be KU, I

[3] Proceedings of the Society for Psychical Research, for June, 1896, pp. 13-17.

ascribed this fragment, with an interrogation point, to King Kurigalzu, whilst I placed the other fragment as unclassifiable, with other Cassite fragments, upon a page of my book where I published the unclassifiable fragments. The proofs already lay before me, but I was far from satisfied. The whole problem passed again through my mind that March evening before I placed my mark of approval under the last correction in the book. About midnight, weary and exhausted, I went to bed, and was soon in deep sleep. Then I dreamed the following remarkable dream. A tall, thin priest of the old pre-Christian Nippur, about forty years of age, and clad in a simple abba, led me to the treasure chamber of the temple on its southeast side. He went with me into a small, low-ceiled room without windows, in which there was a large wooden chest, while scraps of agate and lapis lazuli lay scattered on the floor. Here he addressed me as follows: 'The two fragments which you have published separately upon pages 22 and 26, belong together, are not finger rings, and their history is as follows: King Kurigalzu (ca. 1300 B. C.) once sent to the temple of Bel, among other articles of agate and lapis lazuli, an inscribed votive cylinder of agate. Then we priests suddenly received the command to make for the statue of the god Ninib a pair of earrings of agate. We were in great dismay, as there was no agate as raw material at hand. In order to execute the command there was nothing for us to do but cut the votive cylinder into three parts, thus making three rings, each of which contained a proportion of the original inscription. The first two rings served as earrings for the statue of the god; the two fragments, which have given you so much trouble, are portions of them. If you will put the two together you will have a confirmation of my words. But the third ring you have not yet found in the course of your excavations, and you never will find it.' With this the priest disappeared. I awoke at once, and immediately told my wife the dream, that I might not forget it. Next morning—Sunday—I examined the fragments once more in the light of these disclosures, and to my astonishment found all the details of the dream precisely verified in so far as the means of verification were in my hands. The original inscription on the votive cylinder read: 'To the god Ninib, son of Bel, his lord, has Kurigalzu, pontifex of Bel, presented this.'

"The problem was thus at last solved. I stated in the preface that I had unfortunately discovered too late that the two fragments belonged together; made the corresponding changes in the 'Table of Contents,' pp. 50 and 52; and it being not possible to transpose the fragments, as the plates were already made, I put in each plate a brief reference to the other. [Cf. Hilprecht, "The Babylonian Expedition of the Uni-

versity of Pennsylvania," Series A, Cuneiform Texts, Vol I, Part 1, "Old Babylonian Inscriptions, chiefly from Nippur.] H. V. Hilprecht."

Upon the priest's statement that the fragments were those of a votive cylinder, Professor Hilprecht makes the following comment:

"There are not many of these votive cylinders. I had seen, all told, up to that evening, not more than two. They very much resemble the so-called seal cylinders, but usually have no pictorial representation on them, and the inscription is not reversed, not being intended for use in sealing, but is written as it is read."

Then there follows a transliteration of the inscription in the Sumerian language. Mrs. Hilprecht's statement is as follows:

"I was awakened from sleep by a sigh, immediately thereafter heard a spring from the bed, and at the same moment saw Professor Hilprecht hurrying into his study. Thence came the cry, 'It is so, it is so!' Grasping the situation, I followed him and satisfied myself in the midnight hour as to the outcome of his most interesting dream."

Signed, "J. C. Hilprecht."

A few weeks after the occurrence of this curious dream, there appeared a difficulty which Professor Hilprecht was not able to explain. "According to the memoranda in our possession, the fragments were of different colors, and, therefore, could have scarcely belonged to the same object. The original fragments were in Constantinople, and it was with no little interest that I [Mr. Newbold] awaited Prof. Hilprecht's return from the trip which he made thither in the summer of 1893. I translate again his own account of what he then ascertained.

"In August, 1893, I was sent by the Committee on the Babylonian Expedition to Constantinople, to catalogue and study the objects got from Nippur, and preserved there in the Imperial Museum. It was to me a matter of the greatest interest to see for myself the objects which, according to my dream, belonged together, in order to satisfy myself that they had both originally been parts of the same votive cylinder. Halil Bey, the director of the museum, to whom I told my dream, and of whom I asked permission to see the objects, was so interested in the matter that he at once opened all the cases of the Babylonian section, and requested me to search. Father Scheil, an Assyriologist from Paris, who had examined and arranged the articles excavated by us, before me, had not recognized the fact that these fragments belonged together, and consequently I found one fragment in one case and the other in a case far away from it. As soon as I found the fragments and put them together, the truth of the dream was demonstrated *ad oculos*—they had, in fact, once belonged to one and the same votive cylinder.

As it had been originally of finely veined agate, the stone-cutter's saw had accidentally divided the object in such a way that the whitish vein of the stone appeared only upon the one fragment, and the larger gray surface upon the other. Thus I was able to explain Dr. Peters' discordant descriptions of the two fragments."

There are, says Mr. Newbold, two especial points of interest in this case, the character of the information conveyed, and the dramatic form in which it was put. The apparently novel points of information given were:

1. That the fragments belonged together.
2. That they were fragments of a votive cylinder.
3. That the cylinder was presented by King Kurigalzu.
4. That it was dedicated to Ninib.
5. That it had been made into a pair of earrings.
6. That the "treasure chamber" was located on the southeast side of the temple.

We have a *point de repère* for the treasure chamber part of the dream, in the fact, that Dr. Peters, as far back as 1891, had told Professor Hilprecht of the discovery of a room in which were remnants of a wooden box, while the floor was strewn with fragments of agate and lapis lazuli.[4] The other points in the dream may be accounted for by the direction in which Professor Hilprecht's thoughts had been travelling, or they may not; I must confess to thinking they cannot all be so accounted for.—ALICE BODINGTON.

NOTE.—I would advise anyone interested in the subject of subconscious reasoning in dreams, to read at length the account given by William A. Lamberton, Professor of Greek in the University of Pennsylvania, of a dream in which he solved *geometrically* a difficult problem which he had attacked from its algebraic and analytic side. The *point de repère* here seems to have been a blackboard which had formerly had a functional use in the room, but which had been painted over, the black still showing through the white paint. Professor Lamberton, on opening his eyes one morning, about a week after he had determined to banish this insolvable problem from his mind, saw upon this blackboard surface a complete figure, containing not only the lines given by the problem, but also a number of auxiliary lines, and just such lines as without further thought solved the problem at once.

"I sprang from bed," says Prof. Lamberton, "and drew the figure on paper; needless to say, perhaps, that the geometrical solution being

[4] Two curious cases of the dramatic form taken occasionally by dreams will be found on p. 18, Proceedings S. P. R. for June, 1896.

thus given, only a few minutes were needed to get the analytical one." (Sub-Conscious Reasoning, Proc. S. P. R. pp. 11–13).—A. B.

"The Mimetic Origin and Development of Bird-language," and "The Evolution of Bird-song."—When one considers how many people are thinking at the same time, it does not seem strange that two persons, though widely separated and totally unknown to each other, should sometimes think not only of the same subjects but also follow in the same direction and practically at the same time, the same lines of original thought and investigation. Some of these duplicated ideas are of value in commerce; others are mere metaphysical speculations, possibly suggested by the same incidents; but at this late stage in the knowledge of natural history it does appear unusual that two people in different hemispheres and observing totally different species of animals should have simultaneously pursued independently the same far-reaching but novel line of speculative thought.

A few days ago, Mr. J. E. Harting called my attention to an article in THE AMERICAN NATURALIST, entitled "The Mimetic Origin and Development of Bird-language," which appeared in that journal for March, 1889. He did so because I had lately written a book on the subject, *The Evolution of Bird-song* (London, A. & C. Black, May, 1896), and because the article in question discussed some of my themes. I have just finished a perusal of the article, which was indeed rather exciting, since in nearly every paragraph I found an anticipation of some theory or observation of my own, which I had theretofore believed to be original. In fact, any one reading the article and afterwards reading *The Evolution of Bird-song*, would think that I had borrowed without acknowledgment a good many ideas thrown out by Mr. Samuel N. Rhoads, the writer of the essay in question. However, I am able to prove that in 1888 I had already made investigations on exactly the same lines as Mr. Rhoads, and had recorded the results of them in writing. In the summer of 1887 I began to make systematic records of the imitations I heard from imitative wild British birds, and in the course of this study various themes were attacked, such as "the influence of combat," "the influence of the love-call," "family-voices," and "the influence of imitation," etc. I wrote essays on these themes and sent them to the late Professor Harker, F. L. S. and some of them to Mr. S. S. Buckman (now an eminent geologist), with whom I had many conversations on the subjects in question. In 1890 these observations appeared, in a highly condensed form, in *The Zoologist*, in two papers entitled "The evolution of bird-song," and published respectively in

July and August of that year. I had then of course no idea that my seemingly daring suggestions that mimicry had attuned the cries of birds to their environment, had been confirmed or anticipated in America by a writer who, whatever may be the value of his deductions, is obviously an acute observer. My papers in *The Zoologist* were severely handled last year by a writer who certainly had never heard of Mr. Rhoads' article; and this year the same writer, in favorably criticising in *Nature* my new book, employed a few congratulatory words upon my having allowed certain of my former conclusions to drop into the background.

Although Mr. Rhoads and I were working on the same track, he will I am sure allow that I have gone into the subjects in much greater detail than would be possible within the limits of an article, unless it occupied the whole of the magazine in which it appeared. Mr. Rhoads traces the origin of certain tones to noises produced by the elements, such as the bubbling of air through mud, the murmurs of streams, the sibilant sounds caused by branches being rubbed against each other by the wind, the cries of the victims of predacious birds, the croakings of amphibians, and the moaning of wind in hollow trees; and I adopted the same position, though quoting different instances, in relation to each of these features. He touches on heredity, in a way that would suggest the working-out of the theme in much the same way as I have attempted to do it. In his suggestion of the original use of the voice in "hissing or choking sounds," and in my surmise that the voice was "evolved from a toneless puffing indicative of anger, or from snorts or grunts accidentally caused," in support of which idea some pertinent evidence may be adduced, we have both, I think, advanced somewhat from Darwin's proposition that involuntary and purposeless contractions of the muscles of the chest and glottis, due to excitement of the sensorium, first gave rise to the emission of vocal sounds (*Expression of the Emotions*, pp. 83, 84).

In order to show, however, that I have not been limited to only those themes which Mr. Rhoads treats in so terse and yet so attractive a style, I would mention merely the "contents" of one of my chapters, in which some side-issues are dealt with, as follows:

"Songs are generally uttered by males: exceptions—Not until birds have attained full size: exceptions—Most frequently at morning and evening: influence of weather—Tendency to rise in pitch with vehemence—Only small birds properly sing—Singers arboreal birds generally—Effect of living amid foliage: on size, hearing, and voice—Accent in songs—Singers clad in sober hues—Devel-

opment of the eyes in detecting danger—Necessity of leisure—Labours of parent-birds—Laborious and stealthy birds habitually poor in song—Flight in song: for purposes of display—Fluttering of wings a means of address—Ventriloquism—Singing in chorus."

The study of bird-song, on the lines indicated by Mr. Rhoads and myself, is so new and so delightful a pursuit that I hasten to ask readers of THE AMERICAN NATURALIST to peruse his paper once more; for it is in America, so richly blessed with birds, that such investigations can be most easily developed, and that they promise the most accurate and helpful results.—CHARLES A. WITCHELL.

A Note on Dr. Herbert Nichols' Paper (Amer. Nat., Sept., 1896).—Readers of Dr. Herbert Nichols' paper on my article entitled "A New Factor in Evolution" will understand that its intemperate spirit should rule out all reply. I may say, however, that Dr. Nichol's "home thrusts" are all directed at my view of pleasure and pain, which he considers, quite mistakenly, the point of my paper. On the contrary the "factor" is entirely the influence of the individual's adaptations on the course of evolution; not at all the particular way in which the individual makes its adaptations. I took pains to reiterate this distinction in the paper, saying (AMER. NAT., 1896, p. 542-3) "So far we have been dealing exclusively with facts . . . . . without prejudicing the statement of fact at all, we may inquire into the actual working of the organism in making its adaptations. . . . . Before taking this up, I must repeat with emphasis that the position taken in the foregoing pages, which simply makes the fact of ontogenetic adaptation a factor in (race) development, is not involved in the solution of the farther question as to how the adaptations are secured." So I see absolutely no point in Dr. Nichols' criticisms.

The other question, which involves pleasure and pain, is discussed in the latter part of my paper; but it is not that, but my book which Dr. Nichols attacks with the grossest misunderstanding. I do not at all believe the main things which he attributes to me; first, the position that there are no pain nerves, and second, that there is a "psychic factor" which is an "efficient cause" in evolution. Psychologists know Dr. Nichols' hobby and allow for his intemperateness.

J. MARK BALDWIN.

Princeton, Sept. 25, 1896.

MICROSCOPY.[1]

Methylen Blue.—A few points observed in the use of Erlich's methylen blue method by the investigators in the Marine Biological Laboratory at Woods Holl, Mass. may be of general interest.

This method has been successfully applied during the past summer to the study of the nervous system in a great variety of forms including vertebrates, crustacea, annelids, echinoderms and tunicates.

Ehrlich's *intra vitam* methylen blue prepared by Grübler was used for staining the nerve tissues. The stain was applied by injecting a 1–½ per cent solution of the methylen blue made in normal salt solution, into the blood vessels, body cavity or lymph spaces or by immersing small animals or excised pieces of nerve tissue in a weak solution.

The method of application and strength of the solution were determined by experiment for each animal and tissue. During the action of the stain, the animal or tissue was kept as nearly as possible in its normal condition. Everything seems to depend on keeping the tissue alive, and in bringing the stain in contact with it in a solution of a strength suitable for obtaining the best results.

The abundant supply of oxygen to the staining tissue was of great importance in some cases, while in others it seemed to make little difference.

It was found, as suggested by Dr. C. Huber, that animals which live in the dark, stain better in the dark than in the light.

The relaxation of the tissues by the use of chloroform or chloral hydrate seemed to be more favorable for the staining of some elements of the nervous system, while others did not stain which stained in the unchloroformed animal.

It was found that recently caught and perfectly normal animals stained more satisfactorily than those which had been kept in confinement for some time, unless under very favorable conditions.

In the case of the dogfish, active animals were killed by decapitation. The stain was applied by injecting a 1–½ per cent solution of the methylen blue into the blood vessels for the central nervous system and by immersing small pieces of nerve tissue in a weak solution of the stain for the sense organs.

The length of time required for the *intra vitam* staining varied widely, annelids requiring 4–5 hours, while dogfish only require 1–½ hours, either by injection or by immersing the tissue in the stain.

[1]Edited by C. O. Whitman, University of Chicago.

When small transparent pieces of tissue were to be examined, they were fixed in a saturated solution of picrate of ammonia in distilled water from 2–4 hours and were then mounted in a mixture of equal parts of pure glycerine and distilled water to which a small quantity of picrate of ammonia is added. When opaque or large pieces were fixed in this way they were sectioned by the freezing method. After fixing in the picrate of ammonia, the tissue was placed in a saturated solution of sugar for one hour and was then transferred to a piece of blotting paper to remove the syrup from its surface. It was then placed in a thick solution of gum arabic for fifteen minutes and then transferred to the plate of the freezing microtome where it was frozen by means of liquid carbonic acid. The sections were mounted in dilute glycerine as in the other case. The principal advantage of this method is its rapidity, but neither serial sections nor those of equal thickness can be obtained.

In order to obtain serial sections by the paraffine method, the tissues were fixed in Berthe's Fluid.[2]

For vertebrates:	For invertebrates:
Molybdate of ammonia, 1 gram.	Molybdate of ammonia, 1 gram.
Distilled water, 10 c. c.	Distilled water, 10 c. c.
Hydrochloric acid, 1 drop.	Peroxide of Hydrogen, ½ c. c.
Peroxide of Hydrogen, 1 c. c.	

A different formula is used for tissues of invertebrates as less oxygen is required than for vertebrates. The fixing fluid must be cooled on ice before placing the tissue in it. After remaining in the cold fixing fluid for from 2–4 hours the tissue is thoroughly washed with cold water which generally takes about two hours although it has been continued for twelve hours without injury.

It is necessary to remove all the molybdate of ammonia by thorough washing if permanent preparations are to be secured.

The tissue is then passed rapidly, ten to fifteen minutes in each, through the ordinary grades of alcohol to absolute, all being kept cold with ice. The tissue should be left in the absolute alcohol for about two hours at a freezing temperature and the alcohol be changed several times. The stain is dissolved by dilute alcohol at ordinary temperatures.

Dr. Huber's plan of placing the tissue directly in cold absolute alcohol on removing it from the water and changing several times for a period of two hours, gave good results.

[2] Archiv. f. Mikros., Anat. Bd. 44, Heft 4.

After thorough dehydration the tissue is placed in xylol for 12–24 hours and changed several times. It is then imbedded in paraffine in the usual way.

The most complete and in every way satisfactory staining of the sensory nervous system was obtained by two or three injections of a ½ per cent solution of Erlich's methylen blue at intervals of from 15 to 20 minutes, both with vertebrates and invertebrates, as suggested by Semi Meyer.[3]

The tissues relaxed after the first injection so that more fluid was introduced by the second and third injections than by the first.

The use of chloroform was found to be wholly unnecessary by this method. Meyer uses a very strong solution of B. X. methylen blue, 5 per cent to 6 per cent, in water.

The paraffine sections should generally be quite thick (45–60 $M\mu$).—A. D. Morrill, Hamilton College, Clinton, N. Y.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

American Association for the Advancement of Sciences (continued from page 779).—Prof. A. S. Packard was elected to represent the Association on the American advisory board to coöperate with the American member of the international commission on rules of nomenclature. The following were elected to represent the Association at the International Congress of Geologists to be held in St. Petersburg in September, 1897: Prof. Jas. Hall, Prof. E. D. Cope, Prof. B. K. Emerson, Prof. C. D. Walcott, Prof. W. N. Rice.

The following sectional officers were elected to serve at the meeting of 1897, at Detroit.

Vice-Presidents.—A, Mathematics and Astronomy.—W. W. Beman, of Ann Arbor, Mich. B, Physics.—Carl Barus, of Providence, R. I. C, Chemistry.—W. P. Mason, of Troy, N. Y. D, Mechanical Science and Engineering.—John Galbraith, of Toronto, Can. E, Geology and Geography.—I. C. White, of Morgantown, W. Va. F, Zoology.—G. Brown Goode, of Washington, D. C. G, Botany.—George F. Atkinson, of Ithaca, N. Y. H, Anthropology.—W. J. McGee, of Washington, D. C. I, Social and Economic Science.—Richard T. Colburn, of Elizabeth, N. J.

[3] Archiv. f. Mikros. Anat. Bd. 46. Heft 2, and Bd. 47. Heft 4.

Permanent Secretary.—F. W. Putnam, of Cambridge, Mass. (Office, Salem, Mass.).

General Secretary.—Asaph Hall, Jr., of Ann Arbor, Mich.

Secretary of the Council.—D. S. Kellicott, of Columbus, O.

Secretaries of the Sections.—A, Mathematics and Astronomy.—James McMahon, of Ithaca, N. Y. B, Physics.—Frederick Bedell, of Ithaca, N. Y. C, Chemistry.—P. C. Freer, of Ann Arbor, Mich. D, Mechanical Science and Engineering.—John J. Flather, of Lafayette, Ind. E, Geology and Geography.—C. H. Smith, Jr., of Clinton, N. Y. F, Zoology.—C. C. Nutting, of Iowa City, Ia. G, Botany.—F. C. Newcombe, of Ann Arbor, Mich. H, Anthropology.—Harlan I. Smith, of New York. I, Social and Economic Science.—Archibald Blue, of Toronto, Ont.

Treasurer.—R. S. Woodward, of New York, N. Y.

It was ordered that in future the Vice-Presidents should be called Chairmen of the Sections in the publications of the Association.

Two evening lectures were given to the citizens of Buffalo, viz., The History of the Niagara Gorge, by Dr. J. W. Spencer, and the Results of Cave Exploration and its relation to the Antiquity of Man in North America, by E. D. Cope.

The following papers in the natural sciences were read :

Geology.—*Tuesday, August 25.*—Notes on the Artesian Well sunk at Key West, Florida, in 1895, by Edmund Otis Hovey; The Hydraulic Gradient of the Main Artesian Basin of the Northwest, by J. E. Todd; The true Tuff-beds of the Trias, and the mud enclosures, the underrolling, and the basic pitchstone of the Triassic Traps, by B. K. Emerson; Volcanic Ash from the North Shore of Lake Superior, by N. H. Winchell and U. S. Grant; The Tyringham (Mass.) "Mortise Rock," and pseudomorphs of Quartz after Albite, by Ben. K. Emerson; The Succession of the Fossil Faunas in the Hamilton group at Eighteen Mile Creek, N. Y., by Amadeus W. Grabau; Development of the Physiography of California. Lantern pictures, by James Perrin Smith; Synopsis of California Stratigraphy, by James Perrin Smith; Ancient and Modern Sharks and the Evolution of the Class, by E. W. Claypole.

Wednesday, August 26.—The Discovery of a new Fish Fauna, from the Devonian Rocks of Western New York, by F. K. Mixer; The Cuyahoga Preglacial Gorge in Cleveland, Ohio, by Warren Upham; The Operations of the Geological Survey of the State of New York, by James Hall; A Revision of the Moraines of Minnesota, by J. E. Todd; Origin of the High Terrace Deposits of the Monongahela

River, by I. C. White; Observations on the Dorsal Shields in the Dinichthyids, by Charles R. Eastman.

Zoology.—*Tuesday, August 25.*—On the Entomological Results of the Exploration of the British West India Islands by the British Association for the Advancement of Science, by L. O. Howard; Warning Colors, Protective Coloration and Protective Mimicry, by F. M. Webster; On Life Zones in West Virginia, by A. D. Hopkins; On the Variations of Certain Species of North American Odonata, by D. S. Kellicott; A Case of Excessive Parasitism, by L. O. Howard; Notes on the Occurrence of Dragonflies in Ohio in 1896, by D. S. Kellicott; Scyllarus and Anemonia—A Case of Semi-commensalism, by Edward L. Rice.

Botany.—*Tuesday, August 25.*—The Relation of the Growth of Leaves to the CO_2 of the Air, by D. T. MacDougal; Directive Forces Operative in Leaf Rosettes, by R. N. Day; On Crataegus coccinea and Its Segregates, by N. L. Britton; The Distribution of the Species of Gymnosporangium in the South, by L. M. Underwood and F. S. Earle; Morphology of the Canna Flower, by L. H. Bailey; A Comparison of the Flora of Erie Co., Ohio, with that of Erie Co., New York, by E. L. Moseley; The Point of Divergence of Monocotyledons and Dicotyledons, by C. E. Bessey; On the Bacterial Flora of Cheddar Cheese, by H. L. Russell; The Terminology of Reproductive Organs, by C. R. Barnes; A Comparative Study of the Development of Some Anthracnoses in Artificial Cultures, by Bertha Stoneman.

Anthropology.—*Tuesday, August 25.*—Resolution on the death of Capt. Bourke, Secretary of the Section, followed by a memorial by Washington Matthews.

A Ceremonial Flint Implement and Its Use among the Ancient Tribes of Tennessee, by Gates P. Thurston; Symbolic Rocks of Byfield and Newbury, Mass., by Horace C. Hovey; An Analysis of the Decoration upon Pottery from the Mississippi Valley, by C. C. Willoughby; Brief Description of the Prehistoric Ruins of Tzac Pokoma, Guatemala, by John Rice Chandler; Human Relics from the Drift of Ohio, by W. Claypole; Fresh Geological Evidence of Glacial Man at Trenton, New Jersey, by G. Frederick Wright.

Sociology.—*Tuesday, August 25.*—The Monetary Standard, by W. H. Hale; The Competition of the Sexes and Its Results, by Lawrence Irwell; Fashion—A Study, by S. Edward Warren; Citizenship, Its Privileges and Duties, by Stillman F. Keeland; An Inheritance for the Waifs, by C. F. Taylor.

Wednesday, August, 26-—The Proposed Sociological Institution, by James A. Skilton; What is True Money? by Edward Atkinson; The Value of Social Settlement, by Aaron B. Keeler; The Wages Fund Theory, by Aaron B, Keeler.

Thursday, August 27.—Better Distribution of Forecasts, by John A. Miller; The Tin Plate Experiment, by A. P. Winston; Relics of Ancient Barbarism, by S. F. Kneeland; Suicide Legislation, by W. Lane O'Neill.

Geology.—*Wednesday, August 26.*—Wednesday afternoon was given to a meeting in commemoration of the sixtieth anniversary of of Professor Hall's work in connection with the New York Survey.

The presiding officer spoke briefly in behalf of the Association; and Professor Joseph LeConte, President of the Geological Society, spoke for that Society.

Professor Hall was present and responded in person.

James Hall, Founder of American Stratigraphic Geology, by W. J. McGee; Professor Hall and the Survey of the Fourth District, by John M. Clarke.

Thursday, August 27.—Sheetflood Erosion, by W. J. McGee; Post-Cretaceous Grade-Plains in Southern New England. By F. P. Gulliver.

Thursday Afternoon.—Excursion to Eighteen Mile Creek.

Saturday, August 29.—Excursions.

Zoology.—*Wednesday, August 26.*—Notes Upon Cordylophora, by C. W. Hargitt; Modification of the Brain During Growth, by Susanna Phelps Gage; Structure and Morphology of the Oblongata of Fishes, by B. F. Kingsbury; Differentiation of Work in Zoology—in Secondary Schools, by William Orr, Jr.; Field Work and Its Utility, by James G. Needham; Appendages of an Insect Embryo, by Agnes M. Claypole; Experiments Upon Regeneration and Heteromorphosis, by C. W. Hargitt; The Penial Structures of the Saurians, by E. D. Cope; A Note on the Membranous Roof of the Prosencephal and Diencephal of Ganoids, by B. F. Kingsbury.

Anthropology.—*Wednesday, August 26.*—Indian Wampun Records, by Horatio Hale; Seri Stone Art, by W. J. McGee; The Beginning of Zoöculture, by W. J. McGee; Resolution upon the appointment of a Committee to report on "The Ethnography of the White Race in the United States," by Daniel G. Brinton; Onondaga Games, by W. M. Beauchamp; Meaning of the Name Manhattan, by William Wallace Tooker; Kootenay Indian Place Names, by A. F. Chamberlain; Kootenay Indian Names of Implements and Instruments, by A. F. Chamberlain; Aboriginal Occupation of New York, by W. M. Beauchamp;

Clan System of the Pueblos, by F. W. Hodge; The Psychic Source of Myths, by Daniel G. Brinton; The Limitation of the Comparative Method in Anthropology, by Franz Boas.

Sociology.—*Wednesday, August 26.*—The Proposed Sociological Institution, by James A. Skilton; The Value of Social Settlement, by Aaron B. Keeler; Human Reciprocity, by Mary J. Eastman; Better Distribution of Forecasts, by John A. Miller; The Tin Plate Experiment, by A. P. Winston; Relics of Ancient Barbarism, by S. F. Kneeland; Suicide Legislation, by W. Lane O'Neill; Crime Against Labor, by Edward Atkinson.

Geology.—*Thursday, August 27.*—Notes on Certain Fossil Plants from the Carboniferous of Iowa, by Thomas H. Macbride; The Making of Mammoth Cave, by Horace C. Hovey; The Colossal Cavern, by Horace C. Hovey; Sheetflood Erosion, by W. J. McGee; Glacial Flood Deposits in the Chenango Valley, by Albert P. Brigham; Origin of Conglomerates, by T. C. Hopkins; Origin of the Topographic Features in North Carolina, by Collier Cobb; The Cretaceous Clay Marl Exposure at Cliffwood, N. J., by Arthur Hollick; Post-Cretaceous Grade-Plains in Southern New England, by F. P. Gulliver; The Eocene Stages of Georgia, by Gilbert D. Harris; The Origin and Age of the Gypsum Deposits of Kansas, by G. P. Grimsley; Geomorphic Notes on Norway, by J. W. Spencer; The Slopes of the Drowned Antillean Valleys, by J. W. Spencer; The "Augen-Gneiss," Pegmatite Veins, and Diorite Dikes at Bedford, Westchester Co., N. Y., by Lea McI. Luquer and Heinrich Ries; Notes on Kansan Drift in Pennsylvania, by E. H. Williams; Preliminary Notes on the Columbian Deposits of the Susquehanna, by H. B. Bashore.

Thursday Afternoon.—Excursion to Eighteen Mile Creek.

Zoology.—*Thursday, August 27.*—The Peritoneal Epithelium in Amphibia, by Isabella M. Green; Presented by Simon H. Gage; The Heart of the Lungless Salamanders of Cayuga Lake, by Grant S. Hopkins; Observations on the Chameleon, *Anolis principalis*, by Geo. V. Reichel; Energy in Animal Nutrition; Relative efficiency of Animals as machines, by Manly Miles; Some Abnormal Chick Embryos, by C. W. Hargitt; On a peculiar Fusion of the Gill-filaments in certain Lamellibranchs, by Edward L. Rice; The Relationships of the North American Fauna, by Theodore Gill.

Botany.—*Wednesday, August 26.*—The development of the vascular elements in Indian Corn, by W. W. Rowlee; Some remarks on chalazogamy, by J. M. Coulter; The habits of the rarer ferns of Alabama, by L. M. Underwood; On the stem anatomy of certain Onagraceæ, by

Francis Ramaley; The point of divergence of Monocotyledons and Dicotyledons, by C. E. Bessey; Notes on the pine inhabiting species of Peridermium, by L. M. Underwood and F. S. Earle; Reaction of leaves to continual rain fall, by D. T. MacDougal; Studies in nuclear phenomena, and the development of the ascospores in certain Pyrenomycetes, by Mary A. Nichols; The stigma and pollen of Arisæma, by W. W. Rowlee; Notes on the genus Amelanchier, by N. L. Britton; Remarks on the northern species of Vitis, by L. H. Bailey; On the formation and distribution of abnormal resin ducts in Conifers, by Alex. P. Anderson; The development of the cystocarp of Griffithsia bornetiana, by Arma A. Smith; Notes on the allies of the sessile Trillium, by L. M. Underwood; On an apparently undescribed Cassia from Mississippi, by C. L. Pollard; A bacterial disease of the squash bug (*Anasa tritis*), by B. M. Duggar; What is the bark? by C. R. Barnes; Embryo-sac structures, by J. M. Coulter; Some Cyperaceæ new to North America, with remarks on other species, by N. L. Britton; Grasses of Iowa, by L. H. Pammel; Ceres-Pulver: Jensen's new fungicide for the treatment of smut, by W. A. Kellerman; On the Cardamines of the *C. hirsuta* group, by N. L. Britton; The relation between the genera, Polygonella and Thysanella, as shown by a hitherto unobserved character, by John K. Small; An apparently undescribed species of Prunus from Connecticut, by John K. Small; The flora of the summits of King's Mountain and Crowder's Mountain, North Carolina, by John K. Small; Parthenogenesis in *Thalictrum fendleri* by David F. Day; Notes on the family Pezizaceæ of Schröter, by Elias J. Durand; What should constitute a type specimen, by S. M. Tracy; Rheotropism and the relation of response to stimulus, by F. C. Newcombe; Some adaptation of shore plants to respiration, by Hermann von Schrenk; Influence of rainfall upon leaf forms, by D. T. MacDougal; The mechanism of curvature in tendrils, by D. T. MacDougal.

Thursday, August 27.—The Beginning of Zoöculture, by W. J. McGee; Onondaga Games, by W. M. Beauchamp; Meaning of the Name Manhattam, by William Wallace Tooker; Kootenay Indian Place Names, by A. F. Chamberlain; Kootenay Indian Names of Implements and Instruments, by A. F. Chamberlain; Physical and Mental Measurements of Students of Columbia University, by J. McKee Cattell; Anthropometry of the Shoshone Indians, by Franz Boas; Recent Discoveries and Discussions as to Pygmy Races, by R. G. Haliburton; The Papago Time Concept, by W. J. McGee; Notes on the Theological Development of one Child, by Fanny D. Bergen; Certain

Shamanistic Ceremonies among the Ojibways, by Harlan I. Smith; Notes on Certain Beliefs concerning Will Power among the Siouan Tribes, by Alice C. Fletcher; Pueblo Indian Clans, by F. W. Hodge; Mescal Plant and Rite, by James Mooney.

Geology.—*Friday, August 28.*—Glacial Flood Deposits in the Chenango Valley, by Albert P. Brigham; Origin of Conglomerates, by T. C. Hopkins; Origin of Topographic Features in North Carolina, by Collier Cobb; The Cretaceous Clay Marl Exposure at Cliffwood, N. J., by Arthur Hollick; Post-Cretaceous Grade-Plains in Southern New England, by F. P. Gulliver; The Niagara Falls Gorge, by George W. Holley; The Algonquin River, by G. K. Gilbert; The Whirlpool Saint Davids Channel, by G. K. Gilbert; Profile of the Bed of the Niagara in its Gorge, by G. K. Gilbert; Origin and Age of the Laurentian Lakes and of Niagara Falls, by Warren Upham; Correlation of Warren Beaches with Moraines and Outlets in Southeastern Michigan, by F. B. Taylor: Notes on the Glacial Succession in Eastern Michigan, by F. B. Taylor; The Eocene Stages of Georgia, by Gilbert D. Harris; The Origin and Age of the Gypsum Deposits of Kansas, by G. P. Grimsley; Geomorphic Notes on Norway, by J. W. Spencer; The Slopes of the Drowned Antillean Valleys, by J. W. Spencer; The "Augen-Gneiss," Pegmatite Veins, and Diorite Dikes at Bedford, Westchester Co., N. Y., by Lea McI. Luquer and Heinrich Ries; Notes on Kansan Drift in Pennsylvania, by E. H. Williams; Preliminary Notes on the Columbian Deposits of the Susquehanna, by H. B. Bashore.

Anthropology.—*Friday, August 28.*—Finger Prints of American Indians, by Fred. Starr; The Temple of Tepoztlan, Mexico, by M. H. Saville; The Preservation of Local Archæological Evidence, by Harlan I. Smith; Character and Food, by George V. Reichel; Some Indian Rock and Body Painting in Southern California, by David P. Barrows; Recent Explorations in Honduras by the Peabody Museum, by F. W. Putnam; Pueblo Indian Clans, by F. W. Hodge; Mescal Plant and Rite, James Mooney; Finland Vapor Baths, by H. W. Smith; Cupped Stones, by Franz Boas.

Saturday, August 29.—An excursion to Niagara City, Falls and Gorge was given by the citizens of Buffalo. The party took steamboat to a point on the Canadian side above the Falls, and then took trolley down the river. Fine views of the Falls were had. At Lewiston the river was crossed in a steamboat, and the trolley line up the river carried the party past the rapids and the whirlpool, of which magnificent views were obtained. After dinner at the International Hotel, the establishments of the Power Company and of the Carborundum Company were examined, and the company returned to Buffalo.

SCIENTIFIC NEWS.

Dr. G. Brown Goode, Assistant Secretary of the Smithsonian Institution and Director of the National Museum, died in Washington, Sept. 6th.

Dr. Goode was born in New Albany, Ind., February 13, 1851. As a boy he developed a fondness for natural history. In time he prepared for college and entered Wesleyan University, Middletown, Conn., where he graduated in 1870. During his college career he devoted himself assiduously to the study of natural history, to which his tastes inclined, rather than to classical studies. He took an active part in assembling and arranging collections that culminated in the museum now preserved in the Orange Judd Hall. It was while so engaged that he met Prof. Baird at Eastport, Me., and at once became associated as a volunteer in the work of the United States Fish Commission. The acquaintance thus formed with Prof. Baird continued until the death of Mr. Baird and had very much to do with Dr. Goode's subsequent career.

During the winter of 1872–'73 he made his first trip to the West Indies, and collected fish, which he exchanged with the Smithsonian Institution, and on his return, at the invitation of Prof. Baird, he devoted a part of his time to the institution. In 1873 he joined permanently the staff of the Smithsonian Institution, and has since continued in its service, becoming Assistant Secretary in 1887.

Meanwhile, he took an active part in the preparation of the exhibit of the Smithsonian Institution at the Centennial Exhibition at Philadelphia in 1876, and owing to the illness of Prof. Baird, the charge of that work devolved largely on him during the continuance of the fair. At the end of the exhibition the care of the collections that were then given to the United States government was mainly assigned to him, and to him more than any one else is due the present condition of the National Museum.

His volunteer connection with the United States Fish Commission meanwhile continued, and it was through his relation there that he acquired the well earned reputation of being the leading authority on the fishes and fisheries in the United States. It was this that led, in 1880, to his appointment to the charge of the fisheries division of the tenth census. On the death of Prof. Baird, although the entire care of the Museum fell upon him, Dr. Goode was made Fish Commissioner,

and continued in that place, which position he retained only until the the law could be amended, making that office an independent one. His relation to the Fish Commission led naturally to his serving as United States Commissioner to the Fisheries Exhibition in Berlin in 1880, and in London in 1883.

His experience gained at the Centennial Exhibition in 1876 resulted in his being placed at the head of the Smithsonian Institution and National Museum exhibits at the expositions in New Orleans, Cincinnati, Louisville, and Atlanta. He also served in charge of the exhibition of the National Museum at the Columbian Exposition, in Chicago, in 1893.

In all of this work his remarkable genius for museum administration manifested itself, and the success of the national exhibits was largely due to his remarkable abilities.

Of his many publications the most important were in the line of his chosen science, ichthyology. Among them, worthy of commendation, are "The Game Fishes of the United States," 1879; "The Fisheries and Fishing Industries of the United States" (7 vols.), 1884; "American Fishes," 1887, and with T. H. Bean, "Oceanic Ichthyology," 1893. His writings on museum work include, "Plan of Classification for the World's Columbian Exposition," 1890, and 'Museums of the Future, 1890, both of which are quoted as authority the world over. Mention must be made of his interest in genealogy. As a boy he began the preparation of the genealogy of his family, which resulted in 1888 in "Our Virginia Cousins." This led naturally to his being chosen one of the editors of the "Wesleyan Book," and later to his active participation in the founding of the American Historical Association, in the proceedings of which he published "The Origin of the National Scientific and Educational Institutions of the United States" in 1890.

Dr. Goode was one of the founders of the District of Columbia Society of the Sons of the American Revolution, becoming at once one of the officers, and since 1894, its President. He was also a Vice-President of the Sons of the Revolution and a Deputy Governor of the Society of Colonial Wars. In the scientific societies of Washington City he was ever a prominent member, having been President of the Philosophical Society and the Biological Society. He had been President of the Cosmos Club, and was at the time of his death an overseer of the Columbian University, and in many other ways had been actively associated with the intellectual progress of the National Capital.

In addition, he was a member of many of the leading scientific societies, both in this country and abroad, including the Zoological Society of London, the National Academy of Sciences in the United States, and was recently elected Vice President of the American Association for the Advancement of Science for the Section on Zoology.

Dr. Goode had received the degree of Ph. D. from the Indiana University, and that of LL. D. from Wesleyan University, and his services at the Madrid Exposition gained for him the decoration of Isabella.

Dr. Goode was respected and loved by all who knew him, and he was recognized as a fit successor to Professor Baird, the founder of the National Museum.

Josiah Dwight Whitney, Professor of Geology at Harvard University, died at New London, N. H., at the age of 77 years. He was graduated from Yale in 1839, and from that time until his death he was actively engaged in geological research. His field work included a survey of New Hampshire, a geological exploration of the Lake Superior region, and a survey of the mining regions of all the States east of the Mississippi. In 1855 he was appointed State Chemist of Iowa, and was a member of the faculty of the Iowa State University, later also held the position of State Geologist of California. He was appointed Professor of Geology at Harvard in 1860. In time this position was guaranteed him for life in consideration of the gift of his geological library to the museum of that institution. Prof. Whitney was one of our ablest geologists, and like many men of genius he belonged to the genus irritabile. He was an educated musician, no doubt finding that music is useful for "nerves." He had little patience with lay stupidity, and did not always conciliate "the powers that be."

It will be recalled that the Third International Zoological Congress (Leyden, Sept., 1895) appointed an International Commission of five members to study the various codes of nomenclature in use in different countries, with a view to arriving at a more definite international agreement upon the points of difference to be found in these codes. This Commission is composed of Dr. Raphael Blanchard (France), Prof. Carus (Germany), Prof. Jentink (Holland), Dr. Sclater (England) and Dr. Stiles (United States).

It will also be recalled that Dr. Stiles requested the appointment of an American Advisory Committee, to which he might "submit for approval or disapproval all of the questions which he intended to sup-

port in the meetings of the International Commission and with which he might advise regarding concessions to be made or requested in those points upon which American opinion differs from the views held in some of the other countries."

This Advisory Committee has now been completed and is made up as follows:

Dr. Gill, representing the National Academy of Science.
Dr. Dall, representing the Smithsonian Institution.
Prof. Cope, representing the Society of American Naturalists.
Prof. Wright, representing the Royal Society of Canada.
Prof. Packard, representing the American Association for the Advancement of Science.

The September issue of the Western Field and Stream, published in St. Paul, Minn., is noteworthy as presenting a scheme for the protection of the game of the country. Briefly, it contemplates dividing the entire territory of the United States from the Atlantic to the Pacific, into two concessions along the line of the forteith parallel of latitude, or near it, for each of which there shall be uniform laws and uniform close time, the whole to be under the police surveillance of the National association for the protection of game and fish through its multifarious state auxilliaries. The close time for the northern concession will be from January 1 to September 1, and in the southern concession from February 1 to September 1, during which no shooting shall be allowed on any kind of game whatever, excepting that woodcock and shore birds of the order Limicolæ may be shot in August. The general close time for all kinds of inland fishes, recognized as game fishes, to extend from October 1 to June 1, excepting that fishes of the family Samonidæ, may be caught in April and may. These close seasons conform very nearly to the distribution, habitat, and breeding seasons of the various animals which are sought to be protected; and where they do not, especial exceptions may be made, if deemed expedient. The laws which are to dominate will inhere by legislative enactment; uniform in all the states, and coöperative throughout. Emergencies and bodily stress will always stand in plea for exemption from penalty for violation of the laws when well proven.

By private gifts, a Japanese fellowship in economics has been established at the University of Wisconsin, and Mr. M. Shiozawa, of Tokyo, Japan, has been elected to the fellowship for the coming year. Mr. Shiozawa is highly recommended by two distinguished Japanese pro-

fessors, Professor Iyenaga, of the Higher Commercial College, of Tokyo, Japan, and Professor Motora, of the Imperial University, of the same city. He is spoken of as one of the talented young men of Japan, and it is expected he will do a great work for his native country. He has already graduated from a Japanese college, and has published results of his work. He is now on his way to this country to enter upon his studies at Madison.

The members of the Geological Society of France attending the meeting to be held in Algiers, October 6, 1896, will have an opportunity of examining the following localities: October 8–13, Sahel d'alger, Massif de Blida, Médéa; October 14–18, Kabylie du Djurjura; October 19–26, Constantine, Batna and Biskera. In addition to these excursions there will be one preliminary to the meeting. Mr. Brive proposes, October 3–5, to conduct a party to Chélif and Dakra to study the Miocene and Pliocene beds of these districts.

The Brooklyn Institute of Arts and Sciences has in process of construction a Museum of Arts and Sciences. It is proposed to group on one of the four porticos the names of seven great and representative men in science; on another, seven great and representative men in art; on a third, seven great and representative names in philosophy; and on the fourth, seven great and representative names in the realm of the "practicum" or the application of science and art to the so-called material wants of men.

Information has been received that Prof. Daniel G. Elliott, of the Field Museum, Chicago, who is now travelling in Somaliland, has returned to Berbera from Gallas Mountains. He intends to make arrangements at once for exploring the interior of the country. He has been fortunate in securing a good collection of the fauna of the country, including quite rare species.

The well-known naturalist Mr. Charles H. Sternberg has been collecting fossil plants in the Dakota Group in Kansas during the past season, and has obtained a collection of fine specimens which he offers for sale, either as a whole, or by the single specimen.

The first or "general" part of Dr. Richard Hertwig's *Lehrbuch der Zoologie* has been translated by Professor George W. Field of Brown University, and will be published soon by Henry Holt & Co.

Errata of Paper on The Mushroom Bodies of the Hexapod Brain in the August No.

Page 644, line 6, after "Fig" insert I.
" 646, " 31, before "uses" insert and.
" 647, " 11, for "brahlets" read branchlets.
" 647, " 16, insert coma at the end of the line.
" 647, " 31, insert coma at the end of the line.
" 647, " 34, for "Formol-" read formol-.
" 648, " 1, for "which" read a bundle that.
" 648, after "Fig." insert I.
" 649, " 3, for "afferent" read efferent.
" 649, " 19, after "some" insert of.
" 649, in note for "Dujardin" read Dujardin's.
" 649, line 11, for "crythrocephala" read erythrocephala.
" 649, " 12, for "Gehirus" read Gehirns.
" 649, " 12, for "Diehl" read Dietl.
" 649, " 17, for "Ordunung" read Ordnung.
" 649, " 19, for "Fourmics" read Fourmis.
" 649, " 24, for "Retzins" read Retzius.
" 649, " 30, for "trachéátes" read tracheates.
" 649, " 32, for "Mémorie" read Mémoire.

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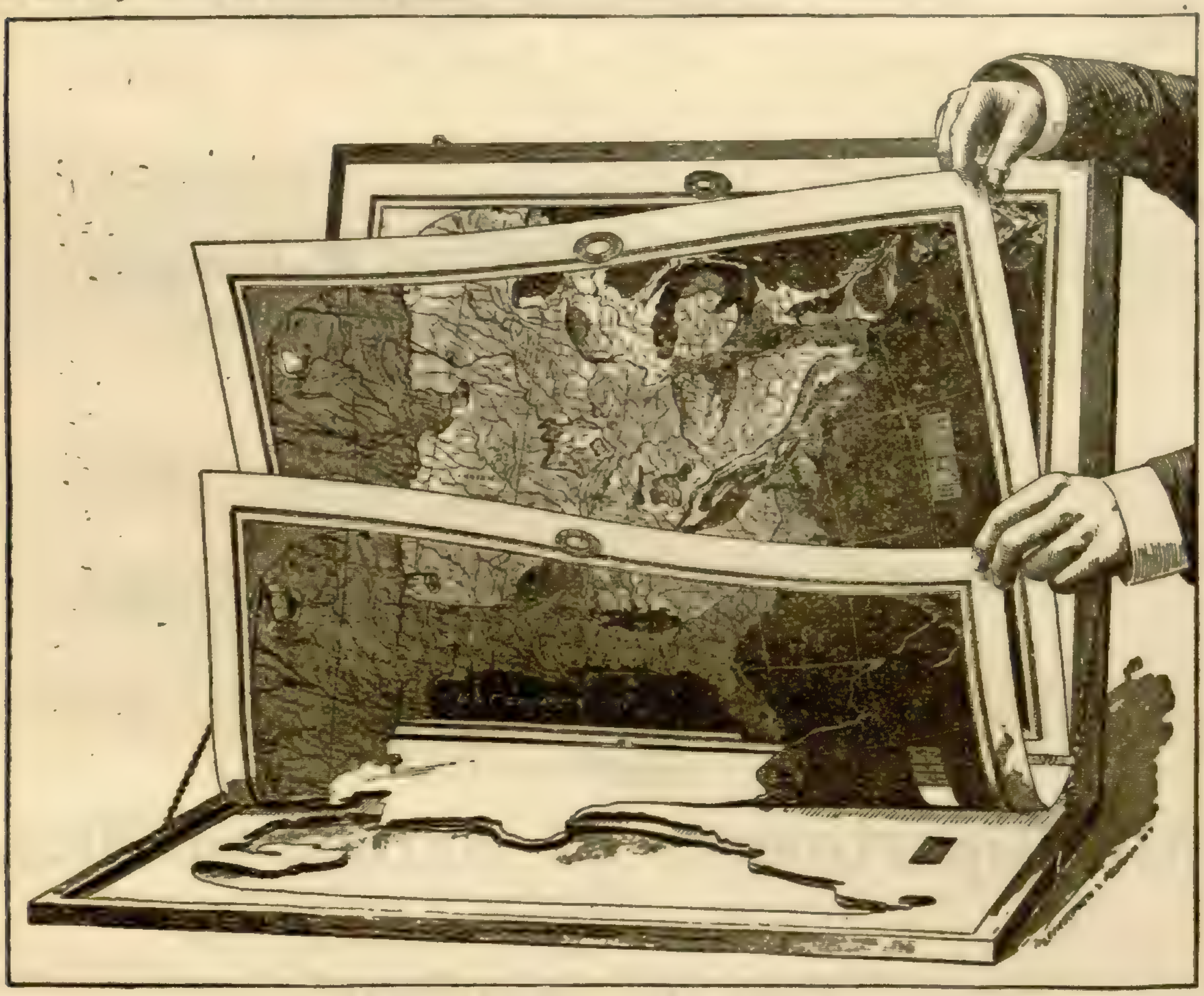

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The following may be mentioned as having contributed to the Volume for '94.

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Rev. Wm. C. Winslow, D. D., L. L. D., Egypt.
Prof. T. F. Wright, Explorations in Palestine.
Henry W. Haynes, Paleolithics and European Archaeology.
Dr. A. S. Gatschett, Indian Linguistics.
Marshall H. Seville, Mexico and Central America.
Hon. James Wickersham, The North West Coast and Eastern Asia.

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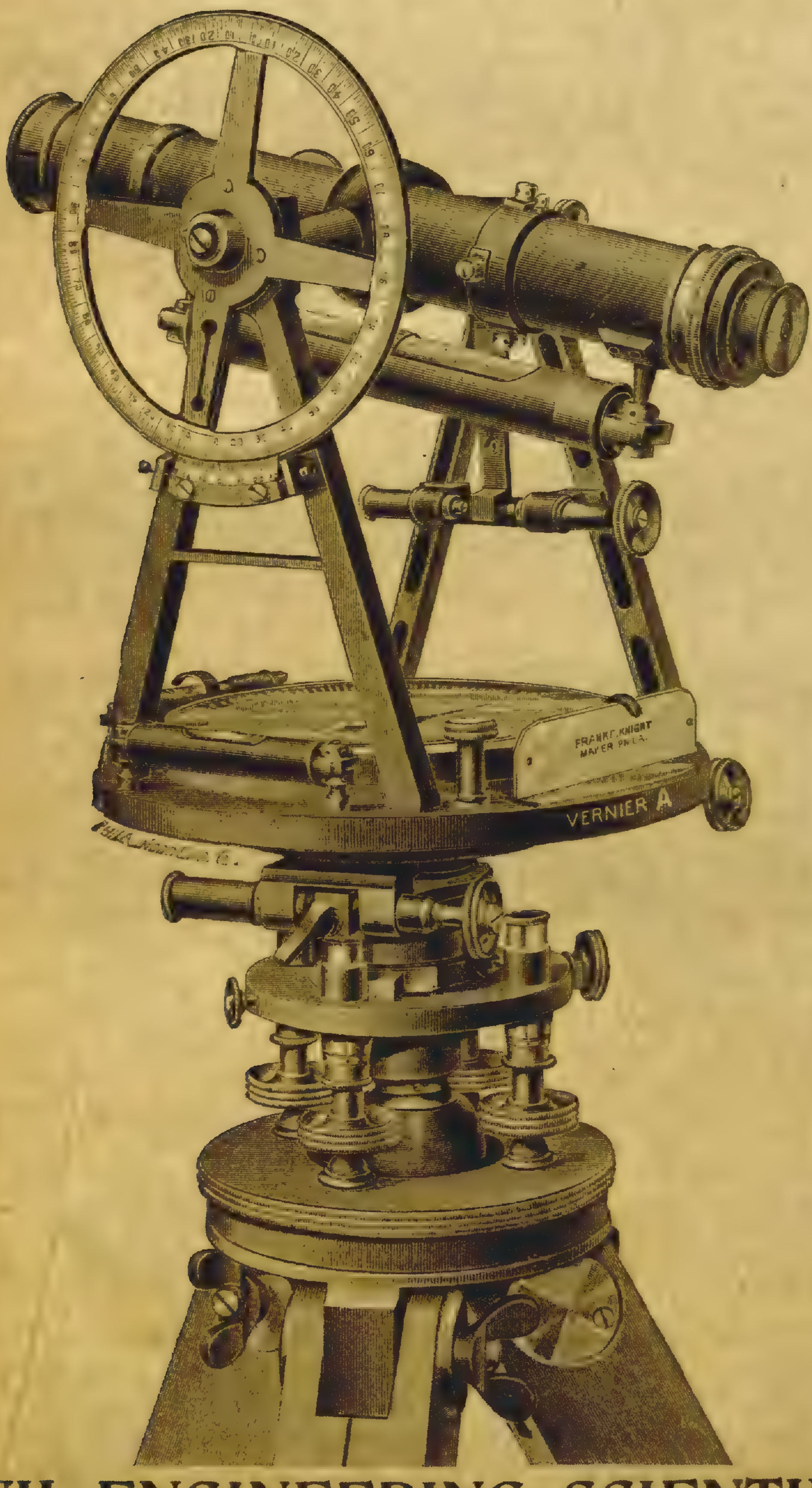

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THE

AMERICAN NATURALIST

VOL. XXX. November, 1896. 359

PINEY BRANCH (D. C.) QUARRY WORKSHOP AND ITS IMPLEMENTS.[1]

BY THOMAS WILSON.[2]

Prof. W. H. Holmes, one of our most accomplished and versatile members, on Nov. 16, 1889, read before our Society an extended report of his investigations in the quartzite boulder quarry at Piney Branch, which was published in the American Anthropologist, January, 1890, pp. 1–26, and is being reprinted in the 15th Annual Rep., Bur. Ethnology. The work described consisted of an excavation up the face of the hill near Piney Branch, in the form of trenches, one 75 feet long and some places 10 feet deep, and other shorter but similar trenches and soundings in the same neighborhood. At the conclusion of his paper, I stood up and complimented him upon the character of the work, and saying that heretofore speculation and theory in the office and with pen and ink, had been employed, instead of actual excavation in the field with the pick and shovel. I congratulated him and the Society upon the new era inaugurated. I said the fact that I did not agree to his conclusions had naught to do with this question and did not prevent me

[1] Read before the Anthropological Society, Tuesday Evening, December 4, 1894.

[2] Curator of Prehistoric Anthropology, U. S. National Museum, Washington, D. C.

from rendering to him the credit for his new mode of investigation, it being the same I had chiefly pursued during my residence in Europe.

Synopsis of the points made in opposition to Mr. Holmes' theories.

1. I concede that Mr. Holmes made a faithful and correct report of his excavations at Piney Branch and of the objects he found there, and I take no exceptions to that part of his paper.

2. But I except to his conclusions. I propose to show that his conclusions are erroneous and that the pretended facts (outside the quarry), on which he based these conclusions, are not facts but assumptions.

3. I propose to show that the objects which he calls "shop-refuse" and which as such should only be found in quarries and on shop-sites, have been equally wide-spread as are the finished implements which he declares to have been the sole aim of the workman in opening the quarry.

4. I propose to show the leaf-shaped implements which he calls "blanks" and which he says were merely material prepared at the quarry for convenience of transportation and to be worked into implements, were themselves finished implements, well-known and commonly used throughout the world in prehistoric times for spears, knives, daggers, etc., with and without handles; and sometimes put to secondary uses, as all other implements might be when broken or the need for their use had passed.

5. I propose to examine the aboriginal village-sites of the District and its neighborhood, in search of the leaf-shaped blades or "blanks" which Mr. Holmes so confidently asserts were carried from the quarry to "other fields" and for other "destinies," and I will show that the number treated as he says, was insignificant when compared with the material procured and the work done.

6. I propose to show the number of caches in the neighborhood to be insignificant; then, that the number of leaf-shaped blades, "blanks," *not in caches*, was also insignificant; then, that those of quartzite which alone could have come from Piney

Branch quarry, are still more insignificant in number, and, finally, that the number of finished implements, such as arrow- and spear-heads, scarpers perforators, etc., *of quartzite* which alone could have been made from Mr. Holmes' "blanks," is even less than insignificant when compared with the mass of implements made of quartz, felsite, argillite, chert, other material than quartzite.

7. I propose to show that, while his facts are assumptions, (always excepting the excavation) he has committed the double error of deducing a wrong conclusion from them, and I will show not only that the leaf-shaped blades (his blanks)—and along with them his finished implements, were not only *not made* from the "double turtle-back," but that they could not be made from it; and that Mr. Holmes' elaborate theory of manufacture as shown by his arranged series of "single turtle-backs," "double turtle-backs," leaf-shaped blades (blanks) and finished implements, being made one after another, each out of the one preceding, will break in two in the middle, because he cannot make the leaf-shaped blades (blanks) out of the double turtle-back without first practically reducing it to its original condition of an unworked pebble.

8. I will contend that the objects found in the quarry should be admitted in evidence and compared with other similar implements in the determination of their age. If this is not done and we are confined for evidence of age, to the quarry and its surroundings, under the assertion that it belonged to modern Indians, then I will attack its antiquity and attempt to show that it may be even more modern than the Indian, and that the quarry might all have been made while digging boulders with which to pave Pennsylvania Avenue in early times. I do not assert this to be true, but if we are deprived of the evidence of the worked implements and driven to surface indications, I will contend that there is evidence (1) of the trench having been entirely filled and carefully levelled so that the contour of the hill shows no trace of excavation which would not have been done by the Indian; (2) that the only other indications of age are the depth of soil on the surface and the size of the trees growing thereon, both of which might have

been accomplished since the laying out of the city of Washington.

9. I make further objection to Mr. Holmes' theory that it assumes jurisdiction of all questions, controverted or not, concerning the age of the quarry, and decides them in such manner as to preclude all further examination. If his theory that the quarry was opened and worked and the implements made by the modern Indian be correct then his decision closes the investigation and passes a final judgment from which there is no appeal.

I.

A portion of the interested public seem to be of the opinion that Mr. Holmes' excavation at Piney Branch was a severe blow to the possibility of a Paleolithic Period in the United States, if it did not destroy that theory altogether. I was not shaken in my faith. My judgment and, I may say without egotism, my years of study of the subject, have given me such an understanding of it as that the excavation at Piney Branch has not caused me to reverse my opinion. If Paleolithic Man existed in America, the traces will be found elsewhere, and a final adverse decision cannot be made upon evidence from a single locality. Therefore I could bide my time.

If the excavation at Piney Branch belonged really to prehistoric times, it is equally favorable to a Paleolithic Period as against it. The investigation reveals nothing incompatible with that theory. The conclusions of Mr. Holmes as announced in his paper were opposed to this, but conclusions are not facts, and renowned investigators have been known to discover many facts, all true, on which they based conclusions which were all error. There are some things in Mr. Holmes' paper not facts but conclusions stated as though they were facts, from which I entirely dissent. Some of these, I propose to examine. In this paper, the facts of discovery, as stated by Mr. Holmes, will be admitted; the errors of argument, theory and conclusion will be combatted.

Prof. Holmes' first five pages (Amer. Anthrop. IV. Jan., 1890.) are employed with the history of the locality; pp. 5 to 9 are filled with a description of the work done and the

method of doing it; at the foot of page 9 commences his description of the art-product. As Mr. Holmes proceeds in his paper with the classification and study of these implements, he announces a primary distinction between those which bear evidence of design and those which do not—p. 11. Every archæologist knows of this as a prime necessity. Every one tries to keep to this distinction. The difficulty is in doing it.

True advancement in science depends on the correctness of the conclusion. To rightly decide this, decides the whole question, not only in this, but in almost every case concerning prehistoric man. Mr. Holmes has no difficulty in this regard. He has supreme confidence in his own ability and admits no possibility of mistake or error. He says same paper, (ibid.) p. 11, "With these distinctions (of design) in mind, the archæologist has but little trouble in recognizing and separating all classes of products and the *uninitiated* with a little careful study may readily learn to do the same. Having handled the products of this shop constantly for a period of several weeks, I have familiarized myself with every variety of form and shade of contour, and *do not feel the least hesitation* in presenting the results of my selection and classification." He then describes his Plate IV, (my Plate XIX), in which "is presented a series of worked stones from this site, which represents every variety of product and epitomizes the entire range of form. Beginning with the boulder *a* from which two chips have been taken, we pass through successive degrees of elaboration, reaching final forms in *k*, *l*, *m*, long leaf-shaped blades. * * If it be asked how I know this series is complete * * the discarded remnants tell the story, * * if every entire flaked tool had been taken from the spot, the record would remain with a certainty that is absolute."

He describes, *seriatim*, the manufacture through the first two stages, and he showed practically before the audience the two processes employed, which consisted of the simple operation of "grasping a boulder in either hand, strike the edge of one against the other so as to detach a flake, then a second and third until the circuit (of the pebble) is completed as shown in *a* to *d* and *n*, Plate IV, thus making a typical turtle-back."

(Fig. 1). The stone was turned in the hand and a second series of blows given on the other side, and the result was a "double-turtle-back," (Fig. 2). The third stage is described in an uncertain and indefinite way, rather out of keeping with the rest of the paper. "If the form (of the first-two stages) developed properly, the work was continued into what I have called, for convenience, the third stage. It consisted in going over both sides a second and perhaps a third time, securing, by the use of small hammers and by deft and careful blows upon the edges, a rude but symmetrical blade. A profile is given at *p.* in Plate IV." It is to be remarked, as a matter of importance, that his manipulations are confined to the first two stages and do not enter upon or pretend to show the third process which reduces the object

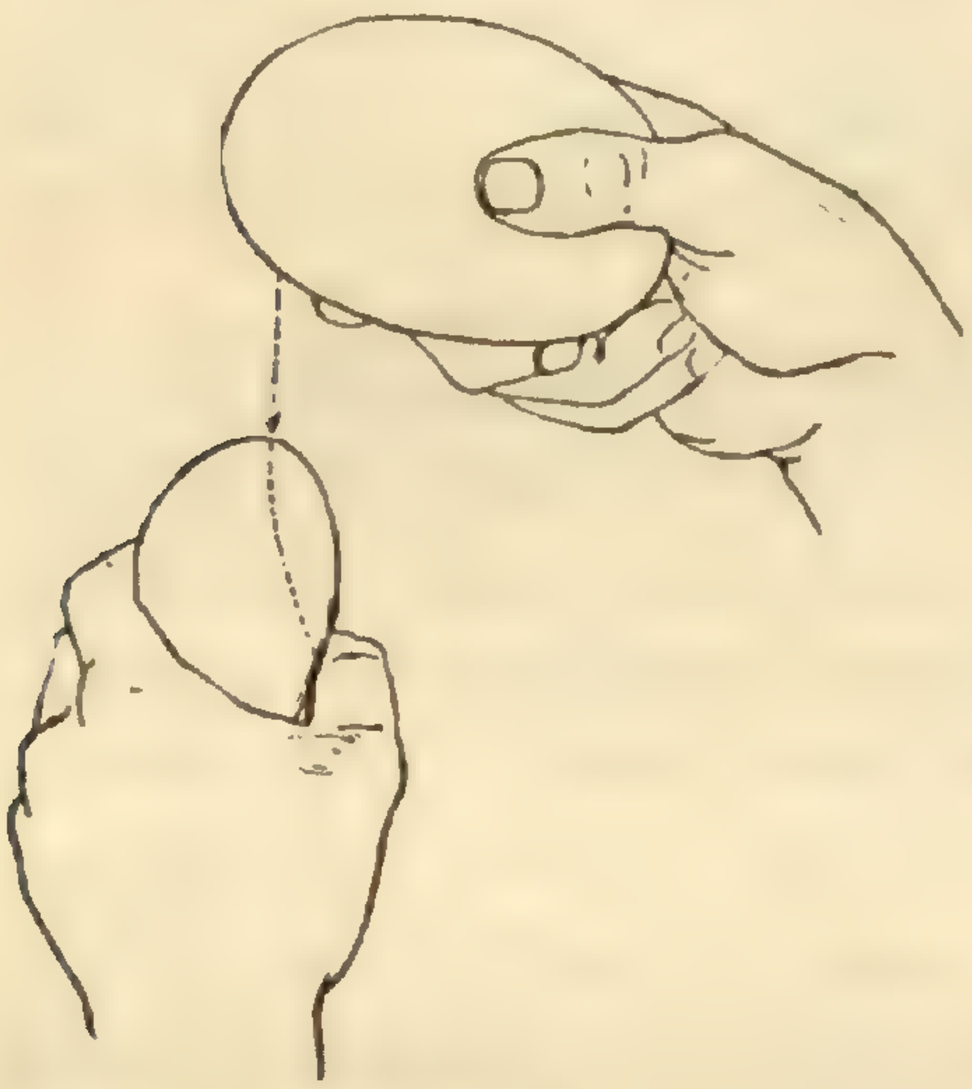


FIG. 1.

Free-hand on direct percussion; first step in shaping an implement from a bowlder. (Prof. Holmes fig. 6).

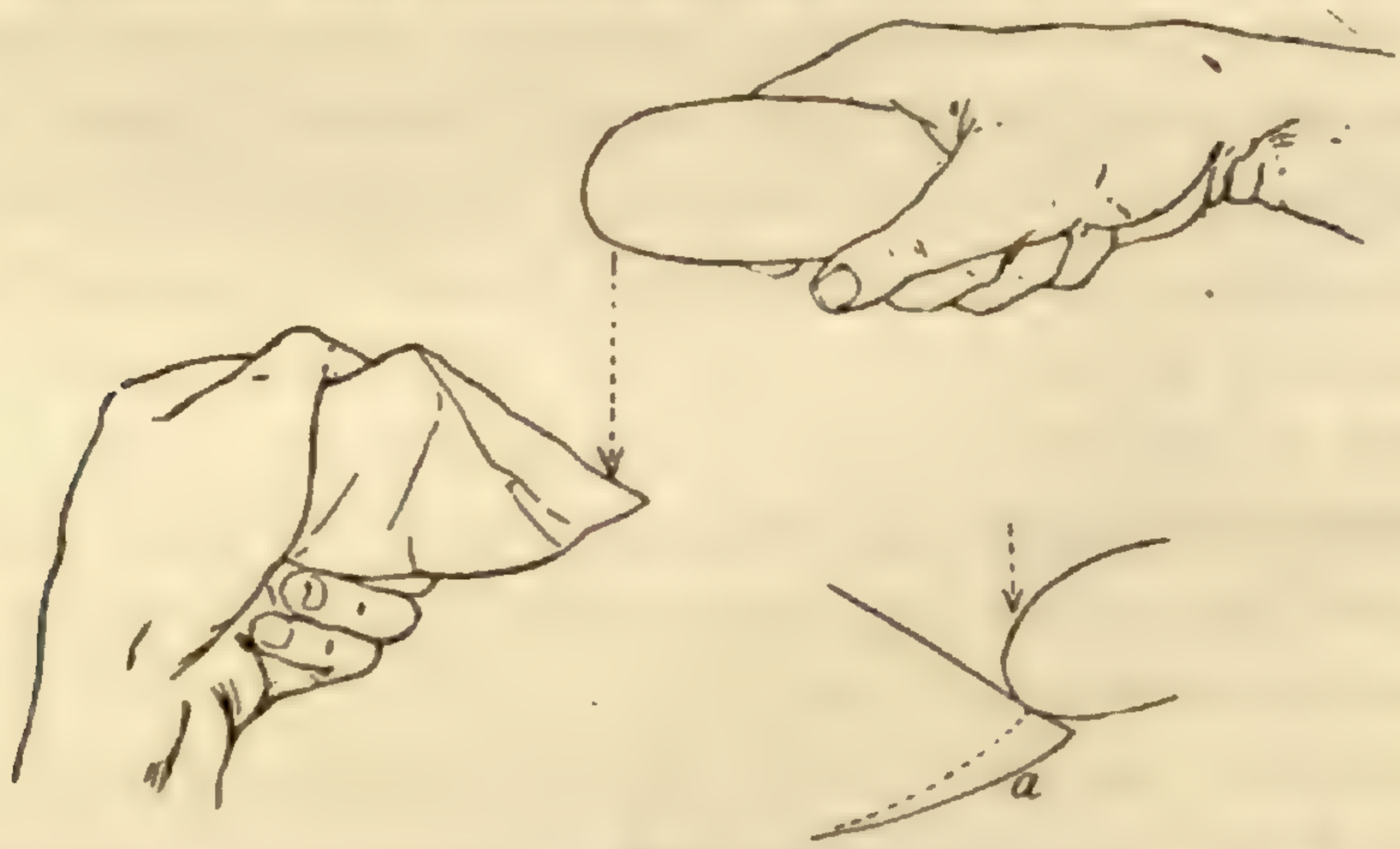


FIG. 2.

Direct percussion; manner of Striking where the edge is sharp. (Prof. Holmes fig. 7).

from a thick and rude implement into "a straight and symmetrical blade less than one-half inch in thickness." There his process fails, and he has not, nor do I believe he

can, by any such process as he has indicated, make out of either his first or second stages, the leaf-shaped implement of the third stage. (I will refer to this later).

His notes on Plate IV are descriptive of the processes and products, and in this he speaks with the air of a master, describing with particularity the intentions and desires of the aboriginal maker, telling wherein he was unsuccessful and specifying the causes of rejection.

First stage—one side worked.

"*a.* Boulder with two flakes removed.

b, c, d. Specimen worked on one side only and had *probably been rejected on account of perverse fracture or excessive thickness.*

Second stage—both sides worked.

e. A few flakes removed from the back; *fracture perverse.*

f, g. Carefully worked on both sides, but still excessively thick, *hence the rejection.*

h. Broken by a stroke *intended to remove a prominent hump.*

Third stage—both sides worked.

i. Neat in shape, but with a ridge or hump on the back which *many strokes have failed to remove.*

j. Unsymmetric broken blade.

k, l, m. Thin, neat, broken blades. * * The last specimen of the series, *m,* is, perhaps, *the most advanced form found,* but that *it was not finished* is clear. * * It is highly improbable that we have in the whole series of products of the quarry, here epitomized, *any finished tool,* either whole or represented by fragments. This should *not be regarded as an opinion only; it is a conclusion based upon evidence that cannot be lightly treated* by the scientific investigator."

This is sufficiently positive to admit of no mistake as to the meaning of the writer. It is an *ex cathedra* opinion. It is the conclusion of one who knows, and who knows that he knows. It is the "Thus Saith the Lord" of holy writ. It is the *ipse dixit* of one who speaks by authority rather than the opinion of one who is making his first essay in his new appointment as Archæologist. This short sentence decides off-hand the

the whole question at issue, for it has been my contention that implements such as were described by Mr. Holmes have been found in many parts of the United States and nearly all over Europe, and have in the latter country always been treated as finished implements belonging to the Paleolithic period; and I respectfully submit that my side of the case is not to be overturned by a declaration made with whatever loudness or clamor, or with whatever positiveness and determination, and which, while declared as a fact, is naught but an assertion.

After having described the manufacture, closing with the third process, the making of the leaf-shaped implements, he says, page 13, "Having followed the process to the end, I wish to call *especial attention to the fact*, if my theory be correct, that when this thin blade was realized, the work of this shop and *the only work of this shop was ended.*" I ask—What right has Mr. Holmes to make this statement? What evidence is there of it? How can he know its truth? How can he know that the "turtle-back" may not have been intentional on the part of the workman, as well as the leaf-shaped blade? How can he know aught about the intention of the workman except, as can every other person, from the implements themselves and the condition in which they are found and the objects with which they are associated? Continuing, "The process and the machinery had accomplished all that was asked of them and all that they were capable of accomplishing."

What evidence of truth have we in these assertions? Again, "the neat, but withal rude blades, and they only, *were carried away and to destinies which we may yet reveal*," p. 13. Who knows what blades were carried away, and what blades *only* were carried away? Is this not an unwarranted assumption? "Further work, additional shaping, employed other processes, and *was carried on in other fields.*" Why? How? In what fields? What further work? What additional shaping? What processes? Where carried? And who knows aught about it? If the writer of these statements had been himself a first-cousin of the paleolithic man and personally present with his kinsman at the close of the Glacial epoch, making notes and sketches of the quarry workshop of Piney Branch,

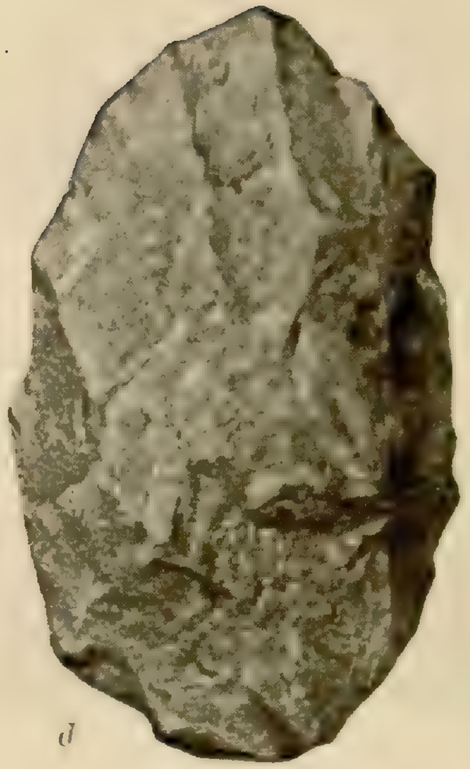
PLATE XIX.

1st Stage—One side worked.

a

b

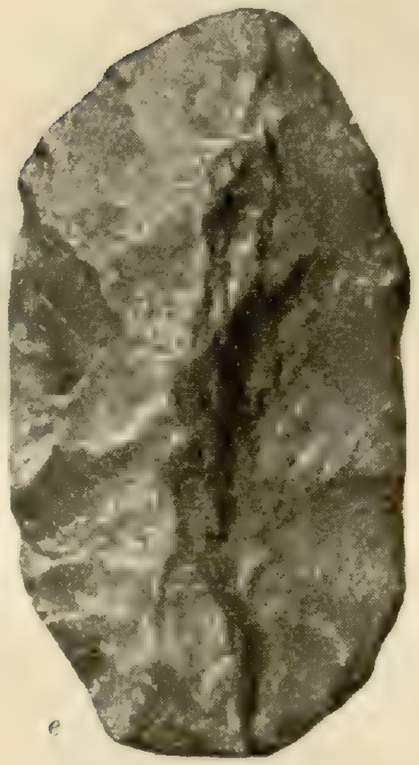
c



d

n

2d Stage—Both sides worked.



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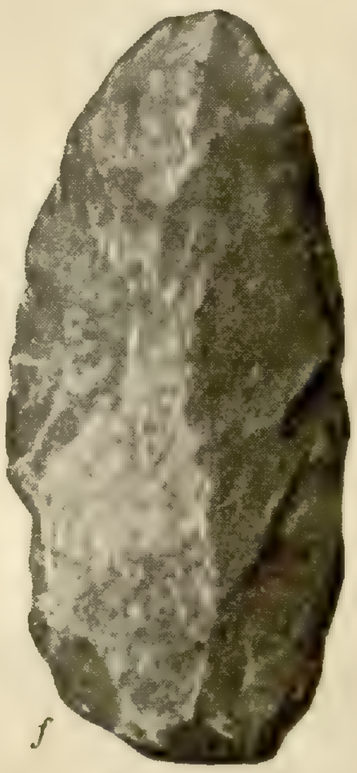

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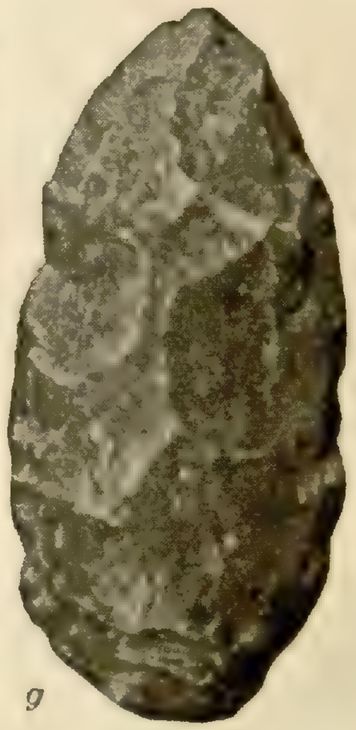
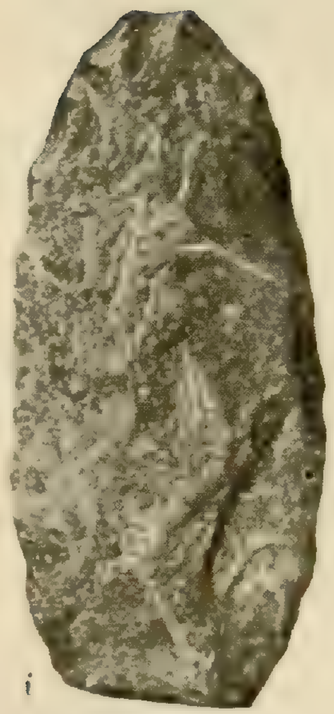


g

h

o

3d Stage—Both sides re-worked.



i

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k

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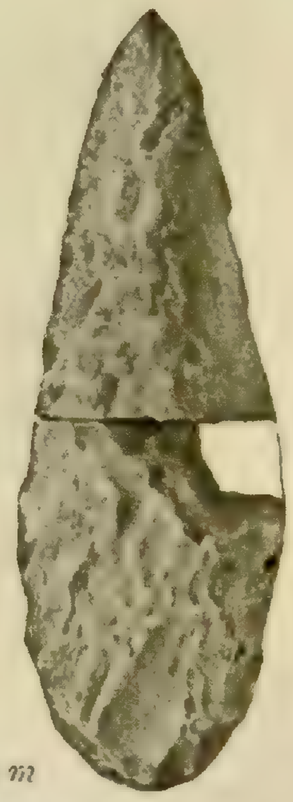
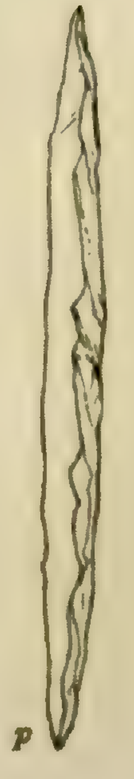


m



p

Series of worked objects from Piney Branch quarry, beginning with the bowlder and ending with the leaf-shaped blade, according to Mr. Holmes PLATE IV.

PLATE XX.

Leaf-shaped implements, half size. *The upper one from France, the lower ones from the United States.*

he could not have spoken with greater positiveness and more certainty of knowledge. One might almost be pardoned, if he continued the reading to the foot of page 13, for believing that the writer considered himself not only omniscient but omnipresent. For he says, "That every implement resembling the final form, made from a boulder or similar bit of rock, must pass through the same, or much the same stages of development just described, whether shaped to-day, yesterday or a million years ago, whether in the hands of the civilized, the barbarian or the savage man."

I envy Mr. Holmes his confidence in his acquaintance with the times and men of high antiquity. Such intimate acquaintance with abstruse and unknown problems is equalled only by Dr. Jock Hornbook's acquaintance with medicine and knowledge of drugs; and he repeats this assumption when he says, "There were no examples of successful quarry-products left upon the ground, all forms available for further shaping or for immediate use *were carried away*, being the *entire products of the shop*, and the *only reward* for the long-continued and arduous labor involved in their production."

In the same strain, in the next paragraph, p. 14, he urges us to keep these facts clearly in mind and then says "it is almost superfluous to expend words in showing that all forms found in the workshop other than the thin blade, accidentally lost, are mere waste," and he declares with a heroic "pang of regret" that he is compelled to drop the turtle-back, single or double, wholly and forever from the category of implements and to consign it to the oblivion of "failures." And, as if to make an end to the discussion and settle the question forever, he, in the next sentence, extends his denunciation to all similar forms throughout the Potomac Valley. We should "cast them at once and without hesitation into the refuse."

In the same manner, and with similar sentences, he settles and decides in an off-hand manner, as though it rested upon some great and well-known law and well demonstrated evidence, the very question at issue, and considers that his discovery has put it beyond the pale of intelligent discussion.

"All forms found in the workshop other than the thin blades accidentally lost are *mere waste ;* * * * this spot is a great workshop where tools were shaped or rather roughed out, and *these things are the failures.*" P. 14.

This is the precise question, decided so dogmatically and with such an *ex cathedra* opinion, which is the point of the discussion; for it has been contended by those who believe in the probability of a paleolithic period in America, that whatever these implements were, they were not failures, not waste or debris, but were intentionally made, and whether they were or were not implements of the paleolithic man, they correspond in a remarkable degree with undoubted paleolithic implements found in nearly every country of Europe.

Mr. Holmes admits, p. 17, "that to a limited extent (he might well have said unlimited) the rude forms—the turtle-back and its near relatives—*are also found scattered over the Potomac Valley outside the shop on the hills.*" He might have added that they were to be found practically all over the United States. I propose to show that they are even more plentifully scattered over the surface of the hills and fields in the neighborhood of Mt. Vernon than around Washington. "This (the above) would seem to conflict with my former statement that all these are failures and were left upon the factory sites," and he adds "It is time therefore, that I should define a stone-age workshop." Mark the adroitness with which he confines the implements within a workshop and yet accounts for their general dispersion throughout the country. He accomplishes it by defining a workshop to extend all over the country. "It (a workshop) is any spot where an individual desiring to make an implement, picks up one or more boulders or bits of stone, proceeds to shape what he desires.

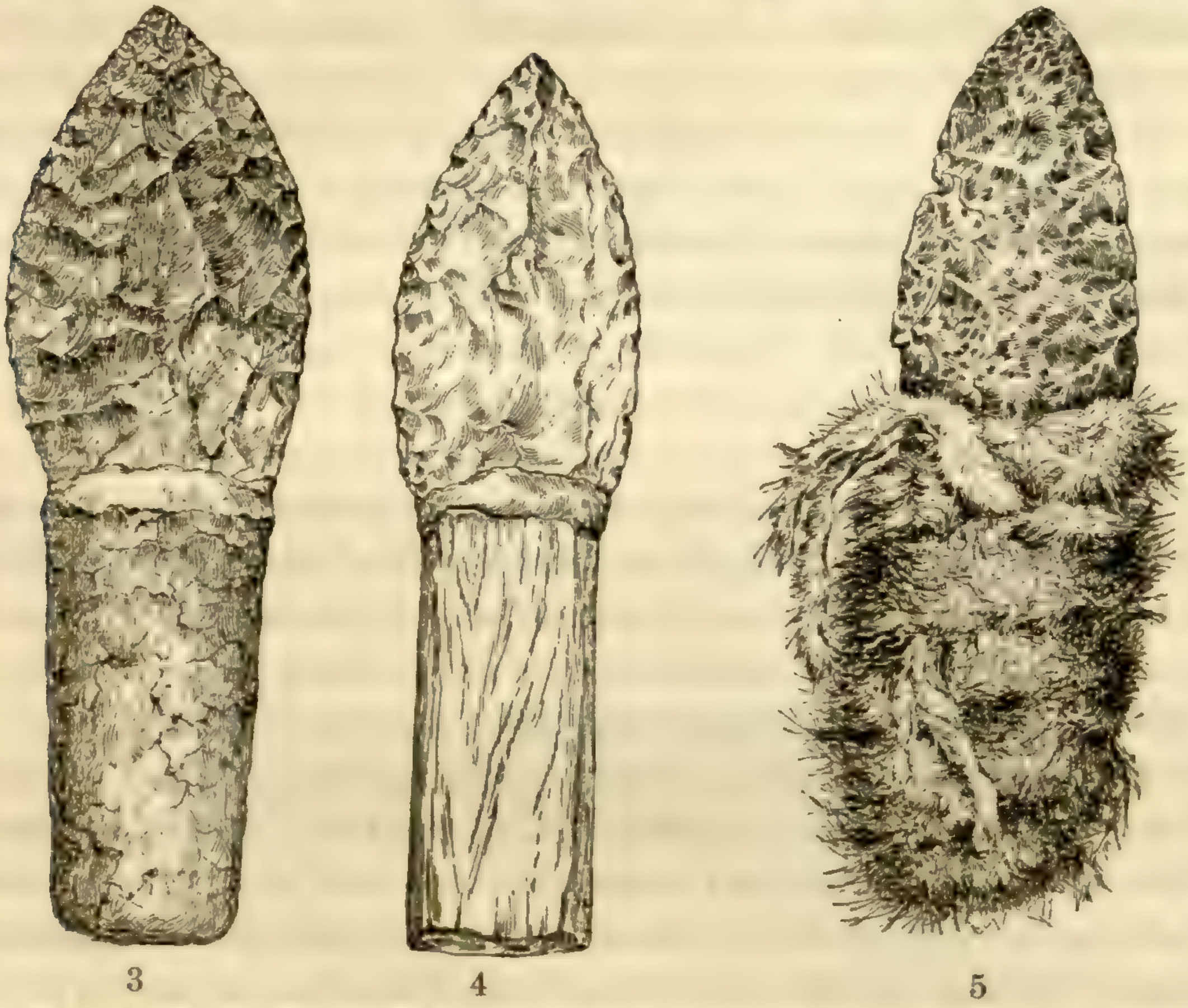
His definition of a workshop is on a par with his other argument. This definition leads him into reasoning in a circle: (1) all turtle-backs are the failures of the workman; (2) this is proved by all the failures being left in the workshops as debris; (3) wherever you find a turtle-back was made, there was a workshop; (4) wherever you find it in a workshop, it was left among the debris. Can any process of reasoning be more

vicious than this? "There is so far no evidence that any inhabitant of the Potomac Valley ever aimed to make by flaking alone any other than the attenuated form," p. 17. "This process leads inevitably to the production of blades *in numbers*, and the supply for the entire year was to be obtained probably within a small fraction of a year, the working period being determined by the season, by tribal movements, or by the limitations of time."

Mr. Holmes disposes these leaf-shaped implements by declaring them (p. 18) to have "been roughed out in numbers to a stage of advancement that made them portable, and at the same time brought them within reach of the processes which he employed in finishing, that they were carried away to the villages and buried in damp earth (cached) until the time came for flaking them into the final forms required by the art. * * * This history of these quarry forms is to be completed by their final distribution among the inhabitants of the various tribes, where we have witnessed the final step in the shaping process —the shaping out of specific forms with a bone tool—and their final adaptation to use and dispersal over the country."

I scarcely know what answer to make to all this. It is so complete and perfect an assumption that one scarcely knows how to make a rational argument against it. I shall refer later to, and show the error of fact contained in this assumption. These leaf-shaped forms have been found in every age of the prehistoric man, in almost every country in the world, they have been of every size—from 16 inches in length down to $\frac{3}{4}$ of an inch. Pl. XX. They are made with all degree of fineness, have preserved their general form and characteristics, and we may imagine in a general way the use for which they were intended. They may have been used as arrow or spear-heads, or they may have been inserted in a shaft or handle and used as spears, or by shortening the handle, as knives; (figs. 3, 4) or they may have been wrapped with hide, bark or grass and this served as a handle (fig. 5). But that they were completed implements ready for whatever use they might have been intended, no prehistoric archæologist of whom I have any knowledge has ever doubted. A duplication and extension

of the uses of nearly every implement may be attributed to the prehistoric man as has been done by the civilized man. No one can prescribe the limits within which a sailor's sheath knife may not be used. A hollow-handled awl is sold in our stores to-day for 50 cents representing itself to be an entire set of tools. The same duplication, changing of purpose and



3 4 5

FIGS. 3, 4. Leaf-shaped implements of jasper, chipped to shape, fastened in wooden handles with pitch or bitumen—to be used as knives, from the Pacific Coast, half size.

FIG. 5. Leaf-shaped blade, chipped, of mattled absidian, wrapped in other skin, used for knife. Collected by Capt. P. A. Ray, from Hoopa Valley, Cal., half size.

adaptation to other purposes, may not have been impossible with the prehistoric man in his employment of these leaf-shaped implements, but that all the leaf-shaped implements, the products of this quarry, and by consequence, all others, should have been but prepared material, wrought from the pebble, to be carried to the home of the man who made them, to be flaked into some unknown and unsuspected implement and then peddled over the country, is an assumption which

has so little foundation as to weaken rather than strengthen his argument. It is only referred to as being on the level with the rest of the paper and to show what a large proportion of it is assumption, and how slight is its foundation of fact.

The method of determining the kind and use of implements, their mode of manufacture and the expected benefit which induced the prehistoric man to expend himself upon them, by comparing him with ourselves, with putting him in our places, or putting ourselves with our knowledge and skill, culture and spirit of invention, in his place, and then deciding everything he did from our standpoint, is one of the errors of modern archeologists and one which leads them far from the right path. This discussion leads Mr. Holmes into the processes of the manufacture of flaked stone tools, and he explains direct or free-hand percussion, declares its limitation, how it was the only method known in early times, and throughout pages 15 and 16, explains the details, giving philosophic dissertations upon the art of stone flaking or chipping and, of the development of the spirit and technology of art required in this work of making leaf-shaped blades. No better answer could be given to this theory than the exhibition of the finely flaked flint implements all prehistoric, coming, some of them, from Scandinavia, Mexico, and a large number belonging to the paleolithic period, found throughout the interior of France in what M. de Mortillet calls the "Solutrèen epoch," M. Reinach and others, the "Cavern epoch." Yet Mr. Holmes has never been able to reproduce one of them or to overcome the difficulties of their fabrication.

(To be Continued.)

THE GEOGRAPHICAL DISTRIBUTION OF BATRACHIA AND REPTILIA IN NORTH AMERICA.

By E. D. Cope.

As is well known, the aggregates of organic beings called faunæ and floræ correspond in part with the natural land divisions of the earth's surface, but not exactly. The first classification of the primary faunæ was proposed by Dr. P. L. Sclater in 1858, as follows:

1. *Paleartic.*—Europe, Northern Africa, Northern and Central Asia.

2. *Ethiopian.*—Africa south of the Great Desert, and Madagascar.

3. *Indian.*—Southeastern Asia and the Malay Archipelago.

4. *Australian.*—Australia with New Guinea and the adjacent islands, New Zealand and Polynesia.

5. *Nearctic.*—North America as far south as Mexico.

6. *Neotropical.*—Central and South America and the West Indies.

Subsequently Dr. A. R. Wallace proposed that the name Oriental be used in place of Indian.

In 1868 Prof. T. H. Huxley proposed that the world's areas be arranged in two divisions, Arctogæa and Notogæa; the former including the Palearctic, Indian, Ethiopian and Nearctic of Sclater, and the latter including the Australian and Neotropical regions. To the last two he added the Novo Zealanian for New Zealand, and he proposes to change the name of the Neotropical to Austrocolumbian.

In 1871 Dr. J. A. Allen proposed the following faunal divisions: I. Arctic Realm; II. North Temperate Realm; III. American Tropical Realm; IV. Indo-African Tropical Realm; V. South American Temperate Realm; VI. African Temperate Realm; VII. Antarctic Realm; VIII. Australian Realm.

In 1874 Sclater modified his system as follows: He retained the term Arctogea in the Huxleyan sense. To the Neotropical region he gave the name of Dendtrogea, and to the Australian

he gave the name Antarctogea, omitting New Zealand and Polynesia, which he constituted a fourth division, Ornithogea.

In 1878 Heilprin proposed the name Holarctic, to include Sclater's Palearctic and Nearctic regions. He also proposed two transitional regions; that of the Old World he called Mediterranean and that of the New World the Sonoran, the latter a term already introduced by Cope for a division of the Nearctic of Sclater.

In 1884 Gill proposed the following primary divisions or realms: 1. Anglogæan (N. American); 2. Eurygæan, or Eurasian; 3. Indogæan; 4. Afrogæan; 5. Dendrogæan, or Tropical American; 6. Amphigæan, or Temperate South American; 7. Austrogæan, or Australian; 8. Ornithogæan, or New Zealand; 10. Nesogæan, or Polynesian. Prof. Gill justly insisted on the importance of fresh water fishes as furnishing definitions of natural faunal realms and regions.

In 1890 Blanford published a system of geographic zoology in which he adopted the primary divisions of Huxley, and divided the Arctogaean region into the following: Malagasy, Ethiopian, Oriental, Aqulonian (= Palearctic and northern part of Nearctic), and Medio-Columbian (S. part of Nearctic).

In 1896 Lydekker proposed the following divisions: I. Notogæic Realm; regions: 1. Australian; 2. Polynesian; 3. Hawaiian; 4. Austromalayan. II. Neogæic Realm; regions: Neotropical. III. Arctogæic Realm; regions: 1. Malagasy; 2. Ethiopian; 3. Oriental; 4. Holarctic; 5. Sonoran. Lydekker makes use of paleontologic evidence in this connection. While this treatment of the subject is important from the point of view of origin, it is often irrelevant, since the distribution of vertebrate life in each geologic age was different from that in each other geologic age.

In an essay on the geographical distribution of North American Reptilia published in 1875, the present writer adopted the first system of Sclater. After a lapse of twenty years, the light thrown on the subject by various investigators suggests the following modifications. In the first place the recognition of the close similarity of the life of the northern regions of the earth, requires more definite formulation than was accorded it in

Sclater's first system, by the union of his three divisions of Nearctic, Palearctic and Indian into one, for which the name Arctogæa is appropriate. The enclosure of his Ethiopian division in it as proposed by Huxley, does not seem to me to be proper, in view of the important types of fishes and reptiles which characterize it; *e. g.*, the Crossopterygian, Dipnoan and Scyphophorous fishes, and the Pleurodire tortoises. In the fishes, indeed, the Ethiopian region has as much affinity with the Neotropical fauna as with any other, in its Characin and and Cichlid families, and in the Dipnoan subclass. The presence of the Dipnoi and the Pleurodire tortoises ally it to the Australian fauna as well. It is for these reasons that Prof. Gill proposes to combine the southern hemisphere realms into a single "Eogaean" division. The northern affinities of the Ethiopian realm are, however, too many to permit us to regard this arrangement as a just expression of the facts. Thus, it has Insectivorous Mammalia, Firmisternial Batrachia Anura, and Cyprinid fishes, none of which are Australian or Neotropical types. The course that remains under the circumstances is to regard the Ethiopian Realm as fully distinct from the other three. The definitions of the four primary divisions are then as follows:

The *Australian realm* is peculiar in the absence of nearly all types of Mammalia, except the *Ornithodelphia* and the Marsupials; in the presence of various Ratite birds, in great development of the Proteroglyph serpents, and absence of the higher division of both snakes and frogs; *i. e.*, *Solenoglypha* and *Firmisternia;* in the existence of *Dipnoi* (*Ceratodus*) and certain Isospondylous families of fishes. On the other hand many of the lizards and birds are of the higher types that prevail in India and Africa, viz.: the *Agamidæ* and the *Oscines.*

The *Neotropical realm* only possesses exclusively the Platyrhine monkeys and the great majority of the humming birds. It shares with the other Southern regions the Edentate and Tapiroid mammals; Ratite, Pullastrine, and Clamatorial birds; Proteroglyph snakes; Iguanid Lacertilia, the Agamids being entirely absent; Arciferous frogs; and Characin, Chromid, Osteoglossid, and Dipnoan fishes. It has but few types of the

Northern regions; these are a few bears, deer, and oscine birds. Insectivorous mammalia, Viperid serpents, and Ginglymodous, Halecomorphous and Cyprinid fishes are wanting, except on the northern border.

The *Ethiopean realm* is that one which combines the prevalent features of the Arctogean realm with the southern hemisphere types already mentioned, together with some found elsewhere only in the Indian region, and a very few peculiar. The two latter classes not being mentioned elsewhere, they may be here enumerated. The region shares, with the Indian alone, the Catarrhine monkeys, the *Elephantidæ*, *Rhinocerotidæ*, Nomarthrous Edentata and Chameleons. Its peculiar types are the *Lemuridæ*, *Hippopotamidæ* and *Protelidæ*, *Cryptoproctidæ* and *Hyracoidea* among mammals, and *Polypteridæ* and *Mormyridæ* among fishes. It possesses in common with the Neotropical realm characinid, cychlidi, and dipnoan fishes, Pleurodire tortoises and Ratite and Trogonoid birds; and differs from it in the absence of arciferous Batrachia and crotalid snakes, and presence of dendraspid, causid, atractaspid and viperid snakes.

The *Arctogean Realm* is characterized by the absence of types conspicuous elsewhere, and by the presence of a few peculiar forms. Among fishes it lacks Dipnoi and Crossopterygia, Osteoglossidæ, Characinidæ and Cichlidæ. It lacks Pleurodire tortoises and Ratite birds. Ginglymodous fishes and Urodele Batrachia are nearly confined to it, merely extending a little over the border of the Neotropical. Its Cryptodire tortoises extend both into the Neotropical and Ethiopian. Anguid lizards are confined to it. It shares most of its Mammalia with other regions. The Insectivora it shares with the Ethiopian, and its deer and camels with the Neotropical. The genus Ursus is very characteristic, one aberrant species only extending into the Neotropical.

From what has preceded it is seen that the primary differences between the faunæ of the realms are to be found to a large degree in the lower vertebrata, the fishes, Batrachia and Reptilia. These forms furnish stronger distinctions than the birds and mammals, owing to their greater inability to traverse

natural boundaries. Neglect of these indications has led to much of the difference of opinion in the question of geographical distribution, which have been founded principally on the conditions presented by the birds and mammalia.

In this system fragments of existing or old continents, which have been subjected to conditions unfavorable to particular forms of life otherwise prevalent in them, are, as in the system of Sclater, disregarded. Thus, islands generally are not regarded as presenting conditions definitive of divisions of the first rank, as was done by Huxley and Gill in the case of New Zealand, and Gill and Lydekker in the Polynesian Islands. The temperate regions of Africa and South America are certainly not separable from the tropical portions, as divisions of primary rank, as was done by Allen, who is followed as to South America by Gill. With equal propriety western North America might be separated from Mississippi and Atlantic North America, on account of the great deficiency of its fish fauna. In estimating faunistic affinities one has to give similarities over a given area more weight than differences, where the differences are only due to absence of types.

Finally, it must be remembered that there are geographic points of transition between all the realms.

I. The Arctogean Realm.

This realm includes three regions, viz.: the Indian, the Holarctic and the Medicolumbian. I have already defined the first two in general terms. The third is the transitional of Heilprin, the Sonoran of Merriam and Lydekker, and the Neotemperate of Townsend. It embraces what is left of the Nearctic of Sclater after the subtraction of the Holarctic. As the name Sonoran has been previously given by me to one of the districts of this region, I have preferred to use for it the name given by Blanford.

The faunal characteristics of these regions may be enumerated af follows:

Indian Region.—Presence of Holostomatous fishes. Absence of Ginglymodous and Halecomorphous and Salmonid fishes. Presence of Cœciliid Batrachia. Absence of Trachystomatous,

Amphiumid, Cyptobranchid, and Arciferous Batrachia. Presence of Agamid lizards, and Anigiostomatous and Viperid snakes. Presence of Phasianid, Eurylæmid, Nectariniid and Pittid birds. Absence of Tyrannid and of several nine-quilled oscine families. Presence of nomarthrous Edentata, of Viverridæ, Hyænidæ, Tupæidæ and Tarsiidæ. Presence of Rhinocerotidæ, Tapiridæ, Proboscidia, and Catarrhine Quadumana, and Anthopomorpha. Absence of Didelphyidæ, Procyonidæ and Scalopidæ.

Holarctic Region.—Absence of Holostomatous and Halecomorphous fishes. Presence of Ginglymodous and Salmonid fishes. Absence of Trachystomatous, Amphiumid and Cœciliid Batrachia, and absence of the Arcifera except the family Discoglossidæ (two species of Hyla excepted). Absence of Angiostomatous and presence of Viperid snakes. Presence of Phasianid, and absence of Eurylæmid, Nectariniid, Pittid and Tyrannid birds, and of several nine-quilled oscine families or subfamilies. Absence of Nomarthrous Edentata, of Viverridæ, Hyænidæ, Tupæidæ, Rhinocerotidæ, Tapiridæ, Proboscidia, Quadrumana and Anthropomorpha (except Homo).

Medicolumbian Region.—Absence of Holostomatous fishes; presence of Ginglymodous and Halecomorphous fishes. Presence of Trachystomatous, Amphiumid, Aciferous and Firmisternial Batrachia, and absence of Cœciliidæ. Presence of Iguanid, and absence of Agamid and Chamæleonid lizards; absence (except three species) of Angiostomatous and of Viperid snakes. Absence of the Indian types of Passeres mentioned, and presence of Tyrannid Clamatores, and several groups of nine-quilled Oscines (Icteridæ Mniotiltidæ, Tanagridæ). Absence of all the specially Indian mammalia, and of the Holarctic Erinaceidæ, and presence of Didelphyidæ (one species), Scalopidæ and Procyonidæ.

In defining these regions I have restricted myself necessarily to types of tolerably high rank, and have not referred to species. This is because species are not generally characteristic of entire divisions, but only of parts of them. One cannot, however, be absolutely exact in such major definitions, since a number of the conspicuous types in each are not universally distributed over these areas.

In comparing the Holarctic with other realms, I have already referred to the number of types which it possesses in common with the Ethiopian, not found in the Neotropical. It has also several in common with the Neotropical, which do not occur in the Ethiopian. These are the Arciferous Batrachia, the Crotalid snakes, and the deer (Cervidæ). The Medicolumbian division of the Holarctic shares other forms with the Neotropical. These are Didelphyidæ and Procyonidæ among Mammalia; Tyrannid, Icterid and Tanagrid birds; Kinosternid tortoises and the Arciferous Batrachian family Hylidæ.

Some of the forms of the Holarctic region are not uniformly distributed over it. Thus the Ginglymodous and Spatulariid fishes only occur in the eastern parts of the eastern and western continents. The same is true of the Silurid genus Amiurus and the Loricate genus Alligator. The Crotalid snakes are not found in the western parts of Eurasia. The Batrachian Cryptobranchidæ have the same distribution.

II. The Medicolumbian Region.

This region was formerly included in the Nearctic of Sclater, and the area thus constituted has the following geographic boundaries. To the south it includes the plateau of Mexico, including the central valley. The Neotropical area bounds it to the east and west, occupying the low-lands or Tierra Caliente to a point 150 miles south of the Rio Grande on the east, (Townsend, Texas Academy of Science, 1895, p. 87), and to Mazatlan, or some point not far from it, on the west. The high land of Oaxaca is its extreme southern outpost. Its northern boundary is thus described by Merriam.[1] The "Boreal" (Holarctic realm) "Province extends obliquely across the entire continent from New England and Newfoundland to Alaska, conforming in direction to the trend of the northern shores of the continent. It gives off three long arms or chains of islands which reach far south along the three great mountain systems of the United States, a western arm in the Cascades and Sierra Nevada, a central arm in the Rocky Mountains, and an eastern

[1] Biological survey of the San Francisco Mountain; N. Amer. Fauna, No. 3, 1890, p. 24.

arm in the Alleghanies, and these interdigitate with northward prolongations of the Sonoran" (Medicolumbian) "province, which latter completely surrounds the southern islands of the Boreal" (Holarctic) "system."

The faunal relations of the Medicolumbian realm may be tabulated as follows:

Agrees with Holarctic in	Differs from Paleartic in	
	Peculiar Forms	Neotropical Forms
Mammalia in general.....	.....	Bassaridæ.
Except.....	....	Procyonidæ.
	Antilocapra.....	Megadermatidæ.
	Mephitis.....	Dicotyles.
	Scalopidæ.....	Didelphys.
Birds, except		Cathartidæ.
		Tanagridæ.
		Icteridæ.
		Clamatores in general.
		Trochilidæ.
	Meleagridæ....	Odontophorinæ.
		Alligators.
		Tiid and Gerrhonotid lizards.
		Iguanid lizards.
Emyd tortoises.....	Chelydridæ.	Cinosternidæ.
Lachesinæ	Crotalinæ	Elapid venomous snakes.
Raniform frogs.....	....	Arcifera.
Scaphiopidæ.....	Plethodontidæ.....	Engystomidæ.
	Amblystomidæ.	
	Desmognathidæ.	
Diemyctylus		
Cryptobranchidæ.....		
	Trachystomata.	
	Necturus.	
	Amphiumidæ.	
Percid fishes.....	Aphredoderidæ	
Cottidæ	Percopsidæ.	
Haplomi.....	Amblyopsidæ.	
Accipenseridæ.		
Spatulariidæ.		
Cyprindæ.....	Plagopterinæ.	
	Catostomidæ.	
Gasterosteidæ.		
Salmonidæ.		
	Amiidæ.	
	Lepidosteidæ.	
Petromyzon		

Baird[2] divided this region into three districts, which he termed the eastern, central and western. The eastern occupied eastern North America to the central plains, where they exceed 800 feet above sea-level. The western included the territory between the Cascade and Sierra Nevada Mountains and the Pacific Ocean. In my paper of 1875,[3] I adopted the eastern, central and western districts (calling the last the Pacific), and proposed two other districts, viz.: the Austroriparian for the Louisianian division of the eastern of Verrill, and the Sonoran for the southwestern and Mexican Plateau faunæ. Merriam, in 1890,[4] proposed a different arrangement. Using the name Sonoran for the entire Medicolumbian Region he divided it into "(1) an *Arid* or Sonoran subregion proper, occupying the table-land of Mexico, reaching north into western Texas, New Mexico, Arizona, and southern California; (2) a *Californian* subregion, occupying the greater part of the State of that name; (3) a *Lower Californian subregion;* (4) a *Great Basin* region, occupying the area between the Rocky Mountains and the Sierra Nevada, extending as far north as the plains of the Columbia; (5) a *Great Plains* subregion, occupying the plains east of the Rocky Mountains, and extending north to the plains of the Saskatchewan; and (6) a *Louisianian* or *Austroriparian* subregion, occupying the low-lands bordering the Gulf of Mexico and the Mississippi, and extending eastward south of the Alleghanies to the Atlantic seaboard, where it reaches as far north as the mouth of Chesapeake Bay." According to his arrangement the Eastern Region of Baird and myself is not mentioned.

This classification may be applicable to birds and mammals; but it is not applicable to the fishes, Batrachia and Reptilia, which are much more exact indicators of the histories of faunæ, owing to their inferior powers of migration. The eastern district or subregion is more nearly allied, from this point of view, to the Austroriparian than the latter is to the Sonoran proper, or arid region. This is due, as Baird previously pointed out,

[2] Amer. Jour. Sci. Arts, XCI, 1866, p. 82.
[3] Bulletin U. S. Natl. Museum, I, 1875, p. 55.
[4] N. American Fauna, 1890, No. 3, p. 24.

to the great difference in rainfall between the part of the continent lying eastward of the 100th meridian and that part which lies west of it. This difference is coincident with a profound difference in geologic age between the regions west of that meridian and the eastern district, the former having a short continental history as compared with the latter.

I, however, agree with Merriam in the abolition of the "Central" as a subregion of Medicolumbia.

The relation of the several zoological divisions to these subregions are as follows: The eastern subregion is the original centre of distribution of all the fishes peculiar to the Medicolumbian region, except only the Plagopterine Cyprinidæ. It is the centre of distribution of all the Batrachia, with the following exceptions: The degenerate types of Trachystomata and Amphiumoidea probably originated in the Austroriparian subregion, and the species of Bufo in the Sonoran. The eastern subregion is also the source of the aquatic Testudinata. On the other hand the Sauria of the eastern and Austroriparian subregions are an overflow from the abundant lizard life of the Sonoran region, excepting the family of the skincs, and the genus Anolis, the latter being of Neotropical origin. The snakes also are mainly Sonoran types, including especially the true rattlesnakes. The copperheads and ground rattlesnakes are on the contrary indigenous to the eastern subregion. The Pacific subregion has close affinities with the Sonoran, but of a largely different kind as to its lizards, while the Batrachia have the character of the eastern types as far as they go.

The distribution of types indicates six principal subdivisions, which I call the Floridan, Austroriparian, Eastern, Sonoran, Western, and Toltecan subregions. The Floridan subregion includes the greater part of the peninsula of Florida, being bounded approximately on the west by the Suwanee River. The Austroriparian subregion extends northward from the Gulf of Mexico to the isothermal of 77° F. It commences near Norfolk, Va., and occupies a belt along the coast, extending inland in North Carolina. It passes south of the Georgia Mountains, and to the northwestward up the Mississippi Valley to the southeastern part of Illinois. West of the Mississippi the bound-

ary crosses Missouri, extends south along the southern boundary of high lands of Texas, and reaches the Gulf at the mouth of the Rio Grande. The Eastern subdivision is the most extended, reaching from the isothermal line of 77° F. north and from the Atlantic Ocean to the elevated plains west of the Mississippi River. Many of its forms extend up the bottoms of the rivers which flow to the eastward through the plains. The Sonoran subregion extends from the limit of the Eastern as far west as the Sierra Nevada, and south, including Nevada, New Mexico, Arizona, Sonora and the Plateau of Mexico, including the State of Chihuahua, and, perhaps, Durango. It does not cross the Sierra Nevada, but includes the entire peninsula of Lower California. It extends northward on the east side of the Sierra Nevada as far as, including the arid region of British Columbia. It occupies the valley of the Rio Grande, and extends into Texas as far as the Rio Pecos. It extends southward in western Mexico as far as Mazatlan. The Western subdivision extends from the Pacific coast to the Sierra Nevada to an uncertain distance on the Lower Californian Peninsula. At the north it crosses the Sierra Nevada, skips the narrow strip of the Sonoran in Washington, and extends to the Rocky Mountains, including northern Idaho and western Montana. The Toltecan subregion includes the States of Guanajuato, Mexico, and the adjacent elevated regions of Michoacan, Oaxaca and Puebla, including the Alpine regions of the southern Sierra Madre. It is probable that another subregion should be added, the Tamaulipan of Townsend. This is a dry region extending from near the mouth of the Rio Grande to the Rio Soto la Marina in the State of Tamaulipas. More information regarding the fauna of this country is desirable.

The faunal peculiarities of these subregions are well marked. The three subregions included in eastern North America differ from all the others in the abundance of their turtles and the small number of their lizards. Prolific of life, this area is not subdivided by any marked natural barriers. Hence, though its species present great varieties in extent of range, it is not divided into districts which are very sharply defined. The warmer regions are much richer in birds, reptiles and insects

than the cooler; and as we advance northward many species disappear, while a few others are added. The natural division of the eastern part of the continent is then in a measure dependent on the isothermal lines which traverse it, which accord also quite closely with its geologic history.

The *Floridan subregion* is distinguished by the presence of several peculiar genera of Batrachia and Reptilia, and by a number of peculiar species. A special feature is the almost total absence of Batrachia Urodela. The genera are:

BATRACHIA:

Lithodytes,
Pseudobranchus.

SERPENTES:

Stilosoma,
Rhadinæa,
Seminatrix,
Liodytes.

SAURIA:

Rhineüra,
Sphærodactylus.

Lithodytes and Sphærodactylus are West Indian Neotropical genera, and Rhadinæa, besides being Neotropical, extends into the eastern part of the Austroriparian subregion. Five genera are then peculiar. The peculiar species will be enumerated later. Several species of mammals are confined to this region. The genera of birds that do not range north of it are, according to Allen:

Certhiola		*Aramus*	Waders.
Zenæda	Pigeons.	*Audubonia*	
Oreopelia		*Phœnicopterus.*	
Starnænas			
Rostrhamus	Raptores.	*Haliplana*	Terns.
Polyborus		*Anous*	

The isolation of the Floridan subregion is due to the fact that the nucleus of the peninsula (which is of Eocene age) was separated from the continent during the greater part of neocene time. If at at any time connected with the Antilles, the period was of short duration.

The *Austroriparian region* possesses many peculiar genera of reptiles not found elsewhere, while the region north of it possesses none, its genera being distributed over some or all of the

remaining regions. The number of peculiar species in all departments of animal life is large. It presents the greatest development of the eastern reptile life. Sixteen genera of Reptiles and eight of Batrachia do not range to the northward, while ninety-nine species are restricted in the same manner. The peculiar genera which occur over most of its area are:

SAURIA:

Anolis.

SERPENTES:

Elaps,
Haldea,
Cemophora,
Tantilla,
Compsosoma,
Farancia.

TESTUDINATA:

Macrochelys.

LORICATA:

Alligator.

BATRACHIA:

Engystoma,
Manculus,
Amphiuma,
Siren.

I have omitted from this list ten genera which are restricted to one or the other of its subdivisions. The *Siren*, the *Cemophora*, the *Anolis*, and the *Alligator*, are the most striking of the above characteristic genera. No genus of lizards is peculiar excepting *Anolis*, which has its greatest development in other than the Nearctic continent. Among Serpents a few genera of Neotropical character extend eastward along the region of the Mexican Gulf, as far as the Atlantic coast, which are not found in any of the northern regions; such are *Compsosoma*, (Central American); *Tantilla*, and *Elaps* (Sonoran). On the other hand, *Abastor*, *Virginia*, *Haldea*, and *Storeria*, embrace serpents which it shares with the Eastern region.

This region is the headquarters of the Batrachia, especially of the tailed forms. The majority of species of the tailless genera are found here, especially of *Hyla* (tree-toads), *Rana*, and *Chorophilus*.

There are no less than nine genera of birds which do not, or only accidentally, range northward of this district. They are, according to Allen:

Plotus,	*Conurus,*
Tantalus,	*Chamæpelia,*
Platalea,	*Campephilus,*
Elanus,	*Helinæa.*
Ictinia,	

All these genera, excepting the last, range into South America or further.

Among mammals, but few species and one genus (*Sigmodon*) are confined to it. *Lepus aquaticus* and *L. palustris*, the cotton rat, etc., and a few others, are restricted by it. The fish fauna is very similar to that of the Eastern region.

The *Eastern subregion* differs from the Austroriparian almost entirely in what it lacks, and agrees with it in all those peculiarities by which it is so widely separated from the subregion. No genus of mammals is found in it which does not range into other regions, excepting *Condylura* (star-nosed mole); but numerous species are confined to it, not extending into the Austroriparian. These number from twenty to twenty-five. Among birds, the following genera are, according to J. A. Allen, shared with the more southern region only: *Quiscalus*, *Siurus*, *Helmitherus*, *Protonotaria*, *Parula*, *Mniotilta*. No genus of Reptiles, and but one of Batrachians (*Gyrinophilus*), is confined to this region; but it shares all it possesses with the Austroriparian. It has but four genera of lizards, viz.: *Sceloporus*, *Chemidophorus Liolepisma* and *Eumeces*.

The *Sonoran subregion* is characterized in the lower vertebrate fauna, by great poverty in fishes, batrachians and tortoises, and abundance of lizards and snakes. Among fishes it lacks the orders Ginglymodi, Halecomorphi and Chondrostei, and possesses only one peculiar group, the Plagopterinæ, a division of the Cyprinidæ. Of usual Holarctic types it possesses only Isospondyli (Salmonidæ) and Plectospondyli; Percomorphi and Nematognathi being absent. The rivers that intersect its central district contain these types, but they must be reckoned as belonging with their bottom lands to the Eastern subregion; the high plains only belonging to Sonoran. The true drainage area of the Sonoran subregion is that of the Colorado.

No genus of Batrachia is peculiar to it, and the following divisions are wanting: Proteida, Trachystomata, Amphiumoida, and all Pseudosauria, except Amblystomidæ (one species). The genus Bufo is the only one that is well represented.

The following genera of reptiles are peculiar to it:

Uta,	*Anota*,
Uma,	*Lichanura*,
Sauromalus,	*Phyllorhynchus*,
Callisaurus,	*Chionactis*,
Dipsosaurus,	*Chilomeniscus*.

It shares the following genera with the Central American subregion of the Neotropical Realm only:

Ctenosaura.
Eublepharis (also in the Indian region).
Phyllodactylus (also in the Columbian Neotropical).
Heloderma.
Hypsiglena.
Salvadora.
Rhinechis (also Holarctic of Eurasia).
Trimorphodon.
Tantilla (also in Brazilian Neotropical).
Cinosternum (also in Brazilian Neotropical).

The following genera of the Sonoran subregion enter the Texan district of the Austroriparian subregion:

Holbrookia,	*Hypsiglena*,
Crotaphytus,	*Rhinochilus*,
Phrynosoma,	*Cinosternum*.
Gerrhonotus,	

Many species are peculiar to this subregion, as will be shown later on.

The *Western subregion* is distinguished by the absence of most of the types of fishes of the humid part of the continent, and the presence of a few. Thus, the Ginglymodi, Halecomorphi and Catostomidæ are absent, while Percomorphi are present. The Batrachian fauna lacks the Proteida, Trachystomata and Amphiumoidea, while Pseudosauria are abundant,

excepting Cryptobranchidæ. All the families of Salientia characteristic of Medicolumbia are present except the Engystomidæ. Among reptiles the genus Charina is entirely characteristic, and Gerrhonotus of the Toltecan and Sonoran faunæ ranges its entire length. It is especially distinguished by the absence of the following genera: First, all of the Iguanidæ exclusively characteristic of the Sonoran fauna, there remaining only *Crotaphytus*, *Sceloporus*, and *Phrynosoma*, which also enter the Texan district of the Austroriparian; by the absence of *Heloderma*, *Ophisaurus* and *Liolepisma*. Among snakes, by the absence of true water snakes (genus Natrix), and the small burrowing Natricinæ, of Opisthoglyph forms, and of poisonous snakes of the genera *Elaps* and *Systrurus*. No genus but *Charina* can be cited as of universal distribution, which is not at the same time found in some other subregion; but several genera occur in one or the other of its districts which do not occur elsewhere. Similarly no genus of birds or mammals can be exclusively assigned to its entire area; but *Chamæa* of the former class and *Haplodontia* of the latter are restricted to particular portions of it.

The *Toltecan subregion* is characterized by the genera it lacks as well as those which it possesses. Thus, it lacks all the genera of Sauria above cited as characteristic of the Sonoran subregion, including those enumerated as passing over into the Austroriparian ecept *Phrynosoma*. It also lacks the following genera of snakes which are found in the Sonoran:

Lichanura,	*Zamenis*,
Pityophis,	*Phyllorhynchus*.
Ophibolus,	
Chilomeniscus,	

From the Austroriparian subregion it differs in the lack of all the numerous genera of Fishes and Batrachia Urodela, which characterize it, excepting only *Spelerpes*. It lacks also the following genera of snakes: *Cyclophis*, *Virginia*, *Haldea* and *Carphophiops*; and *Natrix* is very sparsely if at all represented.

In its positive characters the Toltecan subregion combines certain forms of both the Sonoran and Austroriparian subre-

gions. Of the former character are *Spea*, *Phrynosoma*, *Barissia*, *Gerrhonatus*, *Hypsiglena* and *Salvadora*; of the latter kind, *Spelerpes*, *Liolepisma*, *Osceola*, *Storeria* and *Systrurus*. Characteristic of Medicolumbia generaly: *Amblystoma*, *Rana*, *Sceloporus*, *Eumeces*, *Diadophis*, *Eutænia*, *Crotalus*. Peculiar genera:

Siredon,	*Hemigenius*,
Thorius,	*Epiglottophis*,
Malachylodes,	*Ogmius*,
Conopsis,	*Ophryacus*.

Neotropical genera: *Oedipus*, *Anolis*, *Celestus*, *Atractus*, *Ninia*, *Drymobius*, *Bothriechis*.

(*To be continued.*)

FOSSILS AND FOSSILIZATION.

By L. P. Gratacap.

I.

A fossil, in Paleontology, is any indication of life which has become entirely or partially altered in its substance or condition by the mineral or chemical agencies of its environment. As an "indication" it embraces the widest possible series of remains which have, or could have, any connection with living organisms, from the bones of a vertebrate, the hard parts of an invertebrate, the foliage, fronds, seeds and wood of plants, to the fillings of worm burrows, the tracks of insects, reptiles, mammals, mollusca and crustaceans, and those problematic impressions which have been referred to Medusa, or those by Prof. Hall to the soft parts of an Orthoceras. And it also includes the stony casts formed by the entrance of extraneous mud or silt, sand or chemical deposits within the hard parts of animals upon their death and the disappearance of the soft parts by decay. The hard parts, upon removal by solution, leave the impressions of their interior upon this soft filling which faithfully copies the contour and size of the organism.

These casts form a large group of fossils, and sometimes afford most important information as to the vascular markings and muscular tissues of both crustaceans and brachiopods, though they often perplex the paleontologist by their meagre and unsatisfactory characters. Again, the moulds of exteriors, the phase of preservation complementary to casts must be classed with "fossils." Such impressions often present the superficial ornamentation of shells, and by filling them with soft material as sulphur, wax, or rubber, a reproduction of the original organism in size and form can be satisfactorily obtained. The application of the word "fossil" may be even extended to designate those doubtful evidences of organic life, such as the mixture of the minerals, serpentine and calcite, which have yielded, upon microscopic inspection, some suggestions of organic structure, and with which the famous name of *Eozoon* has been associated, and over which a notable controversy exists to-day. However the term "fossil" is used, its exact meaning from *fodeo* to dig, refers to the most common circumstance connected with the search for fossils, *viz.:* the excavation of rocks or earth, and hence, literally, a *fossil* is a thing *dug out*, implying a past stage of existence in which it has undergone burial and hinting at its subsequent exhumation. This association has no invariable applicability. Fossils are found exposed upon ancient beaches very slightly covered, as in the shell beds at Beauport, New York, where the valves of *Saxicava rugosa*[1] "form a bleached white mass, twelve feet thick, perfectly stratified, and with only sufficient sandy matter to form the lines of division between the strata" (E. Emmons, Geol. N. Y., Pt. IV, p. 129); they are spread upon the surface of wide extents of territory as in parts of Syria where thousands of cardiums, in the form of casts, are seen upon the road and in the fields, from which wagon-loads could be secured, though not a fragment of a shell or a piece of a hinge-tooth, for purposes of identification, are visible (O. Fraas, Aus dem Orient, Pt. II, p. 73). Similarly, in the cretaceous beds of Texas in the

[1] A Lamellibranch or bivalve shell now found living along our coast, from Georgia to the Arctic Ocean; very common from Massachusetts Bay to Labrador, occurring from low-water mark to 50 fathoms or more.

neighborhood of Neu Braunfels, Exogyras and Gryphaeas, are thickly strown over the ground, disengaged by sub-aerial weathering and surface waters from their enclosing marls (F. Roemer, Kreidebildungen von Texas, etc., p. 14, et seq.). Indeed, under some circumstances inhumation quickly destroys fossil remains from the acidic qualities of the soil, as in the neighborhood of Cumanacoa, Venezuela, S. A., as reported by Humboldt[2] (Travels in Equinoctial Regions of America, Vol. I, p. 228, Bohn's Edit.), and we think that, in many instances, fossils have undergone silicification more rapidly when brought under surface conditions where exposed to mineral waters, than they would have if covered in completely, and so removed from the influences of terrestrial circulation.[3]

Yet the word *fossil* is, of course, a distinct reference to a creature living in the past, and, as such, very properly implies entombment of some sort, and, as a fact, fossils are generally embedded in rocks or alluvial and diluvial beds, in clay banks or thinly aggregated beach sands. They may be subsequently exposed by weathering and by removal, but they indicate some sort of initial burial. Their chronological significance indicating the successive phases of animal life in geological time implies a stage-like superimposition with the earliest fossils at the bottom and the latest at the top. As, in fact, such superimposition is only partially, and then locally, perfect—no section of the earth's surface revealing a sheer and consecutive ascension of all the known strata, containing fossils—we constantly find the fossil-bearing rocks forming the surface of wide

[2] Dr. Schwernfurth, in his "Heart of Africa," mentions a soil in which the natives bury their drums, stools, etc., "to give them a permanent blackness." The vegetable acids developed by decomposition in such areas would act more or less corrosively on bone. Mr. J. Richardson tells me that in one year the entire carcass of a cat buried in rich soil had completely disappeared. On the other hand, "the fossil bones of the megatherium of the elephant and of the mastodon, which travelers have brought from S. A., have all been found in the light soil of the valleys and table-lands." Humboldt.

[3] Dr. Otto Kuntze (Nature, Vol. 19, p. 314) has insisted that his observations show that silicified trunks of trees "originate only in air; the siliceous water rises by capillary attraction in the stem, but only on the outside of the trunk does the siliceous solution become solid by drying in the air; from the outside the silicification of the wood cells enters very slowly to the inner part."

regions, and many fossils are in a more desirable condition, as specimens for study, when surface weathering has revealed them from the matrix and brought them into a clean and significant relief, as in the case of chain corals (*Halysites catenulatus*) in the Niagara Limestone of western New York, or when the solvent of carbonated waters has left them accessible, in a friable and open ferruginous sand, as in the weathered rinds and exteriors of the siliceous limestones of the Schoharie Grit of eastern New York.

Fossils represent the hard parts of animals or such portions of their soft parts as have become replaced by mineral materials. The fleshy organic elements of an animal undergo decomposition and disappear, even though enclosed in sediments of great thickness. Water-carrying oxygen finds its way around them and slowly introduces those putrefactive changes which result in disintegration and solution. But in this process, if it is prolonged, there is a substitution of earthy matter, and an infiltration of silt will assume the form of muscular bands, retain the outline of muscular scars and blood-vessels, while a more obscure course of substitution may slowly replace the chitinous, horny, or even fleshy, appendages with silica, iron pyrite, and other mineral species, preserving them with microscopic fidelity. Many forms of animal life, like the Medusæ, Ctenophoræ, Holothurians and Worms, from their soft consistency, are necessarily almost excluded from a representation amongst fossils, being, at the best, only indicated by impressions. The occasional and unusual preservation of the fleshy parts of extinct animals in ice can hardly be regarded as a contradiction of this universal rule.

The various stages of the natural process by which organic beings become fossils[4] may be conveniently regarded as three —first, the *placement* of the object in its position preparatory to fossilization; second, its sepulture or burial, and third, the mineralogical or chemical changes by which it assumes its

[4] The word "fossil" receives a curious application by old writers, especially by the celebrated Werner, who used it to designate any mineral object extracted from the surface of the earth. Thus minerals become fossils, and he speaks of "solid fossils," and he divides them into "hard, semi-hard, soft, and very soft." Also see Pinkerton's Petrology.

permanent form or condition as a fossil. The history and discussion of these three steps form a complete history of fossilization.

The placement of an object will vary in its conditions, according to the habitat of the organism. The most general distinction will be between terrestrial (for the most part, vertebrates) or marine (for the most part, invertebrates) organisms, where the contrasted conditions involve contrasted habits and varying accidents and associations. Upon the surface of the land animals may die in numbers, either upon plains or elevated regions, or in the depths of forests, but the rapid action of decay, or the ceaseless activity of flesh-eating insects may soon dissipate their remains, even when the bony skeleton is imposing and highly developed.

The bones of buffalo remain upon the western prairies for four or five years, at least, in a recognizable condition.[5] In the region about Miles City, Montana, the buffalo abounded as late as 1880. After that time, the remorseless zeal of hunters and the avarice of trade had reduced their numbers and brought them to the verge of extinction. According to W. T. Hornaday, "over the whole of this vast area their bleaching bones lie scattered," and many of these have certainly been exposed to the weather for a period of four years, while their condition when collected warranted the expectation of their remaining sensibly unchanged many more. (See the Extermination of the American Bison, Smithsonian Report, 1887, p. 508.)

Upon the visit of the Challenger to Hard Island, of the McDonald Group in the Antarctic seas, Prof. Moseley observes that upon a sandy glacial plain there were strown "bones of the sea-elephant and sea-leopard, those of the former being most abundant. There were remains of thousands of skeletons, and I gathered a good many tusks of old males. The bones lay in curved lines, looking like tide-lines, on either side of the plain above the beaches, marking the rookeries of old

[5] It is interesting to note that Captain Stansbury observes in his Exploration and Survey of Great Salt Lake, that "carcasses of buffalo left on the open prairie are not unfrequently completely cured, or rather 'mummified' in the sun, so that they seldom exhibit any sign of decay."

times, and teaches of slaughter of the sealers. Some bones occurred far up on the plain, the elephants having, in times of security, made their lairs far from the water's edge. A few whales' *vertebræ* were also seen lying about." In dry or temperate regions, bones resist disintegration indefinitely.

In Canada, at Helena, a vast collection of thousands of buffalo skulls are seen, the enduring vestiges from a slaughter of wild buffalo surrounded by Indians at that place.

The structure of bone in exposures where the climate passes through severe extremes is an element of weakness in their preservation. Winding canals (Haversian canals) traverse the substance of hard bone and secure connection with a series of lacunae, distributed through the substance of the bone, by means of minute tubuli (canaliculi). These tubuli radiate in complex tangles from the lacunae. Sarcodic or marrow-like matter permeates these delicate passages, and, in the Haversian canals, blood corpuscles circulate. In the less dense portions of bones, as the extremities of the leg- and arm-bones, head-bones, etc., a cancellated or cellular structure forms a porous area, while long cavities (marrow-cores) filled with marrow occupy the axes of the longer bones. The decomposition of these organic contents at first weakens bone, and commences an insidious process of splitting. Subsequently the bone becomes filled with water, and the labyrinthine chain of chambers, small and large, are saturated. Cold succeeds, and from the expansion produced by frost the bone is shivered with an incalculable number of microscopic rents. These increase until cohesion is overcome and the bone falls apart.

The mammalian remains may also be placed in soft and water-saturated districts, as swamps and estuaries, lake sides and spring bottoms, whither animals have been attracted by supplies of water or because herbage and prey were equally abundant. Bones and osseous remains of vertebrates buried in rich carbonaceous soil must, to some extent, yield to the corroding action of organic acids. The important roll played by organic acids as agents of decomposition has been recognized. The numerous orders of oxygenated compounds known as acids, and which result from the accumulation of vegetable

matter in fermentable masses, have come to be regarded as most universal and persistent in their influence. It has been shown by Dr. H. C. Bolton that organic acids may be successfully employed for the detection and separation of mineral species, and Dr. A. A. Julien (Proc. A. A. A. Sci., 1879) has gathered together in a comprehensive review such inferences of their geological action as observation of their influence today permits. These acids[6] dissolve iron salts and effect disintegrating effects upon hydrated or soluble quartzes. Such active and omnipresent agencies must exert a very appreciable influence upon animal remains, and in conjunction with carbonated waters must render their preservation precarious. Terrestrial vertebrates, whose remains in the soil of forests or grassy plains would be exposed to the injurious attacks of these vegetable extracts and products, would run some considerable risk of being destroyed.[7] The *placement* of such fossils must be somewhat modified for their effectual integrity. It is true that swamps in whose periods of existence many successions of spagnum layers with the associated accumulation of related and contemporary plants have stored up great quantities of organic debris, have been the repositories of bones, and the great vertebrates, whose bones have become inhumed in their acid-laden depths, have been extracted in a reasonable state of preservation. But it is also true that these heavy bones have worked their way down through the superficial and organic layers to clay marl and sandy bottoms, where they were largely protected from the corrosive action of the humus acids which infiltration and drainage of surface waters would have partially removed.

[6] The vegetable or organic acids are mainly humic, crenic, apocrenic, with which there is an adventitious mixture of oxalic, malic, acetic and fumaric acids, which, according to Julien, are introduced "at least temporarily by the leaves, stems, etc., of most plants, many of which are rich in raphides made up of minute crystals of these acids or their salts."

[7] It is a matter of common knowledge that the bones of goats swallowed by boas undergo solution in the animal acids of their host, so that the calcareous exuviae scarcely equals a tenth part of the original mass of bones, while the so called *album graecum* in the faecas of dogs, hyaenas, etc., represents the residue of digested bones.

The Warren mastodon found at Newburgh, N. Y., in 1845, was embedded in a bed of shell-marl, above which rested a layer of red moss, over which, upon the surface, spread a "thickness of two feet of peat-bog." This specimen was in a very perfect state of preservation, revealing almost the entire skeleton of *M. giganteus*. The Cambridge mastodon was also in an admirable state of completeness, though the nature of the enveloping matrix seemed less favorable. This specimen was taken out at Hackettstown, Warren Co., N. Y., in 1845. The character of the deposit in which it was buried was distinctly organic, consisting of one foot of decayed leaves, six inches of whitish sand mixed with vegetable matter, and a yellow layer, resembling manure, offensively odorous. It would seem a reasonable inference that this large quantity of plant debris would have been unfavorable for the perfect preservation of the bones, and that the organic acids resulting from its decomposition would have aided in their removal. Of course, any protection from the air by the overlying seal of earth, clay, or water, would retard and prevent the oxydation by which the elements of cellulose become converted into acids; and bones immersed in vegetable remains under such circumstances may remain, as practically in this case, exempt from the dissolving agencies of vegetable acids. The Cohoes mastodon, found at Cohoes, N. Y., was taken from a river pot-hole, into which the remains had been carried, and was surmounted by vegetable debris, but had lodged upon an underlying bed of marl and comminuted shale. This skeleton was in excellent condition.

The Ward mastodon, now at the American Museum in New York City, was found at Newburgh, N. Y., in a swampy wet corner of a potato field, and was in a fair state of preservation.

Commenting upon the position of such vertebrate remains, Dr. Warren says: "In nearly all these different spots, the bones have lain at the depth of from five to ten feet below the surface. The same fact is true of deposits near Niagara, described by Sir Chas. Lyell; of those in Virginia, Long Island, the salines of Ohio, Kentucky, and most other places in the western and southern country of the United States.

"The overlying deposits are generally a foot or two of mud, the same thickness of clay, a layer of peat sometimes intervening, and below the clay shell-marl containing everywhere the relics of fresh-water testacea of existing species; some of them perfect, others decomposing. Sir Chas. Lyell, in his geological tour through the State of New York, found at Genessee, the bones of the mastodon in a bed of shell-marl below the peat, corresponding, he remarks, with the situation of the fossil elks of Ireland, generally considered to have been buried in bog-mud or peat swamps, but which, in fact, lie in a stratum of shell-marl."

It seems probable that the enormous quantities of moa bones found in the turbary area at Glenmark, New Zealand, afford some grounds for questioning the destructive influence of vegetable acids. According to Dr. Von Haast,[8] a large swampy tract in Glenmark, covering a depressed region and partaking of the mingled characters of an estuarine and lacustrine basin, contains an incredible number of the skeletons of these great birds. The bones occur here in separated patches or nests, and the impression, made by their distribution, is that of a sudden flight of groups of the birds over this marshy delta which has sunk in places beneath them, and thus entrapped them in constantly increasing numbers. Bones of twenty or thirty individuals, of all sizes and ages, and lying closely packed in spots about five or six feet in diameter, are found with no bones near them, as if, at particular points, the birds had disappeared, one after another, in the enveloping mass of vegetable débris and soft mud. Evidences here are everwhere plentiful of successive freshets by which accumulations of trees, seeds, stems and drift timber have been formed, which, with the growth of bog plants, created a deep vegetable blanket in which the moa bones are immersed. Four to seven feet of pure black peat are succeeded by two to three feet of more impure peat, in which the bird bones are more commonly laid, and under these a hard clay bottom completes the section. It

[8] Geology of Canterbury and Westland, New Zealand, J. Von Haast. See also Ann. & Mag. Nat. Hist., Aug., 1844, Rev. W. Colenso; Transac. New Zealand Inst., Vols. IV, VI, J. Von Haast.

would seem reasonable to expect a plentiful production of humic acid and its allied compounds under these circumstances, and if, as Julien asserts, these acids attack the phosphates of alkaline earths (phosphates of alumina lime and magnesia), the preservation of the moa bones appears either exceptional or contradictory.

In this connection it must be remembered that in all such vegetable infusions a considerable amount of tannin must accumulate, and its astringent action upon the gelatine of bone has a tendency to protect the bone along the interior walls of its cavities and canals. Lyell is at some pains to illustrate, in his Principles, Vol. II, pp. 508–510, the preservative properties of peat, but these illustrations relate more to its antiseptic properties for the preservation of animal tissues. Thus, in a peat-moss in the Isle of Axholm, Lincolnshire, Scotland, a body of a woman was preserved six feet below the surface; bodies of two persons in Derbyshire, England, were kept quite uninjured in moist peat, and pigs were found intact in a peaty soil near Dubuerton, Somersetshire.

There is also a possibly protective action exercised at times by the organic acids themselves when they concentrate upon a nucleus of bony fragments, precipitates of iron oxide or amorphous silica. This is done by their reduction of iron salts forming organic compounds, or by combination with silica in the dissolved silica of the infiltrating streams. The iron is liberated from solution by oxydation and the silica by decomposition, and both iron oxide and soft silica may be thus introduced into the interstices of the bone and serve as agents of induration. It is said by Von Haast that the moa bone layer at Glenmark is somewhat reddish. This may be attributed to ferruginous encrustations. Again, the action of organic acids on such material as the harder class of bone must be somewhat limited by dilution, and the constant percolation of water from surface water-courses and rains must considerably neutralize the corrosive power of the readily dissolved vegetable fluids. Again, these vegetable fluids are quite liberally employed in making defensive combinations with the mineral matter brought to them in complete solution or mechanically

suspended in the streams passing over and through marshes, swamps, bogs and deltas, and are so divested of any destructive power upon bone. And in any case, the elaboration of these acid products which we are considering would be partial or completely suspended at such depths as are usually given for the repositories of vertebrate remains. Yet, however diverted or minimized may be the action of organic acids and carbonated water upon bone, there can be little doubt that it is considerable, and an important means in many cases of imparting to them much fragility or of entirely disintegrating them.

(*To be Continued.*)

THE BACTERIAL DISEASES OF PLANTS:

A CRITICAL REVIEW OF THE PRESENT STATE OF OUR KNOWLEDGE.

By Erwin F. Smith.

(*Continued from p. 804*)

IV.

II. THE HYACINTH (HYACINTHUS ORIENTALIS).

(II) The organism: *Bacillus hyacinthi* (Wakk.) Trev. (1883).

1. *Pathogenesis:*
   (A) Yes.
   (B) Yes (?). The poured plate method was not then in general use. Inoculations were made directly from diseased plants into sterile nutrient fluids, or into tubes of nutrient gelatin, and the resulting cultures may not always have been pure ones, although the writer's own experience has shown conclusively, in case of melon wilt—a somewhat similar disease—that it is often possible to obtain pure cultures in this way, if the culture

media is sterile to begin with and the necessary precautions are taken to exclude surface contaminations and air-borne germs. His experiments were, however, checked and controlled by means of poured plates, whereas Dr. Wakker had advantage of no such exact method. Nevertheless, he seems to have worked with great care, and states positively that although the bacteria were often transferred from diseased plants to the culture media, and also from one tube of media to another, the results were always the same, which could scarcely have been the case were intruding organisms present.

(C) Yes (?). Infections with artificial cultures had not been secured up to March, 1895, and do not appear to have ever been very numerous or very successful. The only experiment which seems to come properly under this head was begun March 4, 1886. The inoculations were made from a liquefied gelatin culture, the fluid being inserted into fresh cuts on the scapes of several (more than five) varieties of hyacinths. In a week all of the scapes began to dry out and soften, from the summit downward; and fifteen days later the greater part of each one was either entirely dry, or soft and flacid. An earlier effort to infect from a bouillon culture failed (Verslag, 1884).

(D) Yes; in part. On microscopic examination of the scapes mentioned under *C* it was easy to determine in them the existence of the yellow disease; but this did not extend into the bulbs. "These experiments [referring to those mentioned under I (5) as well as this one] were repeated and varied with, in general, concordant results."

Conclusion.—Pathogenic nature rendered probable.

Remarks.—As will be seen later on, this organism was imperfectly described, and any bacteriologist having opportunity to repeat and extend Dr. Wakker's experiments should by all means embrace it.

2. *Morphology:*

(1) *Shape, size, etc.*—The bacteria which Dr. Wakker regards as the cause of this disease are represented on his Plate I, Figs. 1–8 (**34**). They are two to four times as long as broad, with an ordinary length of about 2.5 μ. Their form is therefore more or less that of a cylinder, but with rounded ends. They are said to agree tolerably well in size and shape with *Bacterium Termo*. The organism was described as *Bacterium Hyacinthi* in 1883, but was placed under *Bacillus* by Trevisan in 1889. When these bacteria have been in a nutrient liquid for some time a certain number become longer than they were, and now measure 4 μ, while the ordinary length is only 2.5 μ. Later on, as the nutrient matters of the liquid are becoming exhausted, the bacteria diminish in size more and more, and gather into motionless groups, which often have circular outlines and which grow by the accession of new individuals, while the motile bacteria become less and less numerous. Bacteria from the dry slime were found to be only about half the ordinary size, but on placing them in nutrient fluids they resumed their normal size. Examinations were made in hanging drops of nutrient fluid.

(2) *Capsule.*—No mention of any capsule.

(3) *Flagella.*—No mention of flagella. The organism is said to be actively motile in culture fluids. Even those kept for some time in a dry state are said to have acquired motility on placing them in nutrient fluids. In the yellow, viscid slime, as taken from the plant, they are not motile; but motility begins as soon as this is diluted with a $\frac{3}{4}$ per cent. salt solution, or with a suitable nutrient fluid. "After a short time all is life and motion; the bacteria, in the form of straight but very flexible rods, are to be seen moving about actively; individuals in repose are rare. Among the undivided bacteria there are many which are in process of division, and which then show two individuals moving together; these, however, soon separate to continue an independent existence." It will probably be found that the organism is also motile in the plant in early stages of the disease, *i. e.*, before it has multiplied to such an extent as to fill the vessels. Dr. Wakker himself says: "It is evident that

though they exhibit no visible motion in the slime they cannot be entirely without motion in penetrating into the bulb."

(4) *Spores.*—The bacillus produces endospores. Their development and germination was followed with so much care that it appears worth while to give a somewhat detailed account.

"In the cultures already described [those at room temperatures] no spores were found. These had, therefore, to be sought in some other way. They were finally obtained from the liquid cultures by keeping them at a higher temperature. Drops of nutrient fluid containing the bacteria were placed in an enclosure having a uniform temperature, night and day, of 35° C., *i. e.*, at a temperature exceeding the mean temperature of the room by about 20° C. Ordinarily, at the end of ten days, spores appeared, and the characteristic agglomerations of small, motionless individuals did not appear. Subsequently it was found that a temperature of 35° C. was not absolutely necessary for the formation of the spores. In fact, during the summer, when a rather high mean temperature prevailed in the room, some cultures produced spores without artificial heat; but these were never as numerous as those formed in the tubes kept at the uniformly higher temperature of the enclosure. The spores of *Bacterium Hyacinthi* (**34**, pl. I, fig. 1) have that lively bluish brilliancy which is usually so characteristic of the spores of bacteria, and which is caused by the strong refraction of the light. These spores are always a little longer than broad, and form in the interior of the largest rods near the middle, although ordinarily slightly nearer one of the extremities. In consequence of their strong refrangibility it is difficult to decide with certainty whether the rod is swollen around them, as has been indicated for several similar species. This swelling, if it exists, must be very slight, since the spore is not thicker than the bacterium itself. Besides the rods with ripe spores, there are, ordinarily, a great number engaged in forming spores, and these still move about in a lively manner. On the contrary, when the spores have reached full development, the rods which contain them remain motionless and their wall soon disappears, so that the spores become completely free. In this state they

are about 1 μ in length, while their breadth does not exceed $\frac{1}{2}$ or $\frac{2}{3}$ of this size. Each rod produces only one spore. If these spores are allowed to dry on the glass where they have been formed, they may be kept for a long time, and subsequently on placing them in a nutrient liquid the development of new bacteria may be observed. At the time of germination, which is hastened the same as sporogenesis, by an increase of temperature, the spore begins to swell and its cylindric form changes to an ellipsoid. The strongly refractive power is also gradually lost, the middle of the spore first becoming dull while the brilliant gleam still persists more or less at the two extremities (**34**, pl. I, figs. 2 and 3). Here we have the condition which must be considered as the commencement of germination. The wall is split into two portions, which remain united at one side. The central part of the spore from which the refringence has entirely disappeared, is the place where the two halves of the spore open one from the other, and here a baculiform body of slight refringence was observed pushing out (pl. I, figs. 4 and 5). During the growth of this body the sheen also diminishes very greatly at the two extremities of the spore, and soon there is a state which cannot be indicated better than by likening the germinating spore to a hammer, the two portions of the wall of the spore representing the head while the handle is formed by the rod which has issued from the spore (fig. 6). Often after a longer or shorter time, the rod, one end of which is squeezed between the two parts of the wall of the spore, begins an oscillatory movement, and thus succeeds in freeing itself, whereupon it moves through the liquid in the manner common to bacteria, the empty wall being left behind (fig. 6c). In other cases, after escaping from the spore, the young bacterium remain motionless in front of the empty wall for a long time before swimming away. Finally, the rod sometimes drags the empty wall after it (fig. 8). In all cases the rod which has escaped is an ordinary bacterium which soon divides in the manner already described."

The author never found spores in the living hyacinth. This, he says, accords with de Bary's observation on *Bacillus anthracis*, he having never found spores in the living animal.

This absence of spores in the living plant is also in harmony with the fact that in the nutrient liquid the formation of spores begins only when the alimentary substances are exhausted. This, naturally, is never the case in the living bulb. It is not impossible, however, that when diseased bulbs have been entirely destroyed spores may form in the remaining mass if the temperature is favorable. An effort was made to prove that these spores were actually developed from the hyacinth bacillus by allowing a drop of fluid containing them to dry on a slide for some time, and then placing that part of the slide bearing the dry spores in contact with the fresh cut surface of a bulb. In three weeks the yellow disease was discovered in the vessels of the bulb, and it was at once apparent that it had already been developing in these for some time. This experiment was repeated several times, and always with the same result. This, indeed, is not full proof; but when old cultures are used very few vegetative rods are left, and the infection is believed to have resulted principally from the germination of the spores in the sticky fluid that oozes from the cut scales, the bacteria finding their way from this into the vessels.

(5) *Zooglœa.*—No special mention of zooglœa. Possibly the more or less circular or globular groups of motionless rods which commonly appeared in the cultures as they became exhausted are to be regarded as such.

(6) *Involution forms.*—No mention of any involution forms.

3. *Biology.*

(1) *Stains.*—This organisms stains very readily in the most diverse analin colors. The author made a variety of experiments to determine the best method of staining the bacteria in place in the tissues. He obtained the best results with analin browns, especially phenyline brown (Bismark brown), but states that many other colors may be used, *e. g.*, eosine, methyl violet, analin yellows and picric acid. The yellow stains have the special advantage of giving to the preparation almost exactly its natural color. To stain in place, sections made from alcoholic material should be put into a saturated alcoholic solution of the analin brown, left for a few minutes, and then transferred to strong alcohol containing an

extremely small quantity of hydrochloric acid. In this the color rapidly disappears, especially if the fluid is stirred with a glass rod. At the end of a very short time, the length of which varies in different cases, certain parts will be seen to have preserved their color, if the disease is present, while the rest of the section has already bleached. The sections must now be removed immediately to a dish of pure, anhydrous spirits of turpentine, in which they are left until thoroughly penetrated by the liquid. They may then be examined directly or first mounted in Canada balsam, after which they may be kept indefinitely. When the work has been well done the sections will be brown in those parts which contain the bacteria and which were originally yellow, while in all other parts they are colorless.

(2) *Gelatin.*—The culture media was made by adding to water containing glucose and a little meat extract, enough gelatin to give a solid, clear yellow, perfectly transparent mass at ordinary temperatures. This was sterilized by heating from time to time to 100° C. It was then carefully pipetted into tubes which were plugged with cotton, and re-sterilized by heating every day to 100° C., for some days. Pipettes, tubes and cotton plugs had previously been heated to 140° C. Tubes prepared in this way were unplugged, infected with bacteria taken from a diseased bulb (the transfer being made by means of a platinum wire previously heated to redness), quickly closed, and then left at the ordinary room temperature. The organism makes a good growth on gelatin. The gelatin is readily and completely liquefied.

"*Experiment of June 12, 1885.*—The above described operations were made this day, and two days later I saw in all the tubes the gelatin liquefy under the influence of the bacteria. Examination showed that the part not yet liquefied also contained bacteria, so that the latter must first penetrate into the gelatin and then cause its liquefaction. The formation, in the part of the gelatin which is still solid, of white globules consisting entirely of bacteria, served to make this fact very apparent. Bubbles of gas which can only arise from the action of these organisms also developed in it continually. After a short

time the whole mass was liquefied, and the bacteria were found at the bottom of the tube as a thin whitish layer. The liquid is then a clear brown, darker than the original gelatin. The contents is almost odorless.

"This experiment was repeated very often, and always gave the same result, only in subsequent experiments, it happened sometimes that the white globules did not appear. This, however, is not surprising, since I then employed a mixture (glucose, extract of meat, gelatin) of slightly different composition, and since, moreover, the temperature was not always the same. On peut naturellement infecter aussi quelques tubes au moyen de Bactéries prises dans d'autres tubes; cela n'a jamais rien changé aux résultats."

No gelatin roll or plate cultures were made, and the behavior of the organism in stab and streak cultures is not carefully described.

(3) *Agar.*—No account of any experiments on agar media.

(4) *Potato, etc.*—Nothing mentioned.

(5) *Animal Fluids.*—The first artificial medium was made by adding a little meat extract and grape sugar to a decoction of meat which had been kept for some time in spirits, and was freed from the latter by washing and boiling in distilled water. It was then boiled for an hour in an additional quantity of distilled water and the sugar and meat extract added. It remained clear for ten days, was then reboiled, cooled quickly, and a small quantity of the yellow slime introduced, the greatest care being used throughout to avoid contaminations. The second day this fluid became distinctly clouded, and this clouding increased for four days, and then remained the same. The inoculations that failed were from this culture. The slime used to infect this culture came from a single vascular bundle of a freshly cut bulb. It was scraped off on a flamed cover-glass, which was then thrown into the fluid. The organism also grew well in a solution of meat extract to which glucose had been added. This was the fluid culture medium ordinarily employed, and there is no mention of any other. The exact composition of the medium is not given.

(6) *Vegetable Juices.*—None mentioned.

(7) *Salt Solutions and other Synthetic Media.*—No mention of any; but since the organism is not strictly parasitic it is inferred that it can grow and maintain itself for a long time in a variety of organic substances.

(8) *Relation to Free Oxygen.*—The organism is aerobic and probably also facultative anaerobic, although no mention is made of any experiments to determine this point.

(9) *Reducing and Oxidizing Power.*—Peptonizes gelatin.

(10) *Fermentation Products and other Results of Growth:*

(a) *Gas Production.*—Organism produces gas in meat extract gelatin containing grape sugar. Kind of gas not determined.

(b) *Formation of Acids.*—No statement.

(c) *Production of Alkali.*—No statement.

(d) *Formation of Pigment.*—In the vessels of the plant the organism produces a bright yellow color, which is soluble in glycerin, but insoluble in water and alcohol. This pigment became darker on drying. The dextrose, meat extract gelatin became darker colored (clear brown) after liquefaction.

(e) *Development of Odors.*—The organism produces little or no odor either in the plant or in the artificial cultures. This absence of odor may be used to distinguish the disease from other hyacinth diseases, some of which are very malodorous.

(f) *Enzymes.*—Evidently not studied. Organism produces at least two; one capable of peptonizing gelatin, and another which dissolves the cellulose of the hyacinth.

(g) *Other Products.*—None mentioned.

(11) *Effect of Dessication.*—The organism can be kept for a long time in a dry state without dying, *e. g.*, on a glass plate. It shrinks to about one-half normal size, but on placing again in suitable fluids it recovers its former size and makes a new growth. One of these hanging drop cultures was begun in a somewhat different way. The bacterial slime was not taken directly from a bulb but from a glass plate on which it had been placed and dried long before. The slime and the nutrient fluid were then mixed in the same manner as before; but instead of rods 2.5 μ long, the bacteria were now smaller. Moreover, at first they were distributed through the liquid passively,

and a longer time passed than in the other cultures before their own movement appeared. Nevertheless, after some hours, it began, and first as a simple rotation. At the same time it was determined that the dried bacteria had been reduced to about one-half the ordinary size. But the following day they had resumed the ordinary size, and then also showed the characteristic backward and forward movement. From this point on the culture presented the identical phenomena described above. This shows that the bacteria of the *maladie du jaune* can live for a long time in a dry state, and that on drying they are reduced to dimensions comparable to those which they assume in a liquid in which the alimentary substances are becoming exhausted. I infer that this dry mucilage did not contain spores.

(12) *Thermal Relations:*

(a) *Maximum for Growth.*—Not determined.

(b) *Optimum for Growth.*—Not determined. The organism grows at living-room temperatures, and also in the thermostat at 35° C.

(c) *Minimum for Growth.*—Not determined. The natural progress of the disease in the hyacinth fields appears to be slow, and probably low temperatures may have something to do with this.

(d) *Death Point.*—Not determined.

(13) *Relation to Light.*—Not determined.

(14) *Vitality on Various Media.*—Seems to be capable of living for a considerable period in various media.

(15) *Effect on Growth of Reaction of Medium* (*acid, neutral, alkaline.*—No statement.

(16) *Sensitiveness to Antiseptics and Germicides.*—No statement.

(17) *Other Host Plants.*—No mention of any. Some speculation as to origin of the disease, but no facts.

(18) *Effect upon Animals.*—No statement. Probably not tried.

(III) Economic aspects:

(1) *Losses.*—No statement as to the extent of damage done by this disease. The disease is spoken of in one place as the chief subject of his investigations, and in another place the organism is called a "dangerous parasite."

(2) *Natural Methods of Infection.*—Little that is definite can be gathered from Dr. Wakker's writings. The sticky slime which oozes from rifts in the affected leaves is highly infectious, adheres to whatever it touches, retains its vitality for some time, and is readily borne about on light objects. He discusses the possibility of the germs entering through the blossoms, and considers that wounds are more likely sources of infection, because the attacked blossoms would fall off quickly and carry the germs with them. It probably enters the plant through wounds made by man or animals. Dr. Wakker thinks it especially likely to enter through wounds of the scape made in cutting the flower, or through injuries done to the young scales by pulling leaves, or by cutting healthy bulbs with infected knives in process of making incisions in the bulb, or of separating the scales for purposes of reproduction. It is evident, however, from the fact that the greater number of the plants are first attacked at the tip of the leaf, that some other unknown method of infection is the more common one. Dr. W. thinks the infection often takes place very early in the spring and generally through the air, the sticky bacterial exudate from the leaves, etc., being carried to sound plants by wind and rain, or by flies and other insects which frequent the hyacinth fields on warm days (Verslag, 1883). For various reasons Dr. Wakker thinks that the parasite may sometimes enter through the uninjured leaf, *i. e.*, through the stomata, but does not appear to have induced the disease in this way. Wounds are always moist, and the bacterium finds food ready for its use in the dead cells of the wound, whereas if it enters through the stomata it must make its own food from the start. The stomata are also very small, and infection through the uninjured leaf surface is probably uncommon.

(3) *Conditions Favoring the Spread of the Disease.*—Dr. Wakker states that the spread of the disease is favored by wet weather, and that dry weather and continuous sunshine are the best preventives. If the much lessened prevalence of the disease in 1883 as compared with 1882 is to be attributed in part to the precautionary measures taken, it is not less certain that the frequent rains of 1882 did great injury to the plants in this

particular. "In 1883 innumerable were the cases in which I observed that the descending stripe on the leaves was dried out and stopped, so that the bulb was not attacked." The rapidity of the infection depends largely on the temperature, the dampness in the surrounding air, and on the amount of water in the plant itself. The location of the wound might also make a difference.

(4) *Methods of Prevention.*—An inquiry among the growers elicited the statement that there is a great difference in susceptibility. This Dr. W. thinks cannot be denied. Some varieties are very subject; others, in the same beds or gardens, have not been known to be attacked. Many varieties formerly held to be exempt from the disease are now known to be subject; but some remain which have never yet shown the yellow disease, and this cannot be ascribed to mere accident; on the contrary, it can be explained only by assuming that predisposition or readiness to be attacked here plays a prominent part (Verslag, 1883). Anatomically, so far as known, all are alike. Lists of "very susceptible," "less susceptible" and "not susceptible" varieties are given, from which it would appear that single varieties are more susceptible than double ones, and the exemption of the latter is not due to their lesser number. All of the double red varieties and most of the other double sorts are exempt, or but little subject to attack. These lists are based on statements furnished by only seven growers, but include many varieties (Verslag, 1885). Of thirteen varieties said to be very susceptible by several or most of these seven growers only one is double, la Tour d'Auvergne. On this account, difference in receptivity is suggested as a means of combatting the disease. New varieties must not be originated from susceptible ones. Seedlings should be derived from hardy sorts, and by artificial fecundation, the pollen of susceptible varieties being excluded. Otherwise, through the agency of insects, the resulting cross may prove susceptible. The law of heredity is shown still more rigorously in non-sexual reproduction. It is best, therefore, to discard sensitive sorts and try to obtain new ones which are more robust.

In the division of bulbs for propagation the greatest care should be taken never to cut a healthy bulb with a knife which

has been in contact with a diseased plant, at least not until it has been disinfected.

There is another point to which the author desires to call special attention, viz., to the removal of leaves which begin to show signs of the disease at the tip. On May 20, 1883, the diseased leaves were entirely cut away from seventeen hyacinth plants. On September 26th, sixteen of these bulbs were entirely sound, although rather small. The other bulb was entirely decayed; but from what cause, it was no longer possible to determine. Planted in pots these sixteen bulbs blossomed in April, 1884. The following June they were dug up once more, and on cutting them open all were found to be sound. This experiment was tried on many other bulbs, and always with the same success. It was also tried by several horticulturists in their fields with results entirely confirmatory. It is, therefore, certain that the bulb can be preserved by the judicious removal of diseased leaves.

Since the bacteria have always penetrated much further into the leaf than is to be seen with the naked eye, the whole leaf should be removed even when only slightly attacked. The frequent complaint that cutting off the diseased parts does no good, shows that not enough attention has been paid to this. Of course, when the bulb is already infected, cutting off the leaves amounts to nothing (31).

Finally, it goes without saying that the debris of diseased hyacinths should not be left in the field or near it, as one might be tempted to do on account of its value for manurial purposes. All such debris should be thrown into a deep ditch and disinfected with quick lime.

Remark.—Considering the time when this piece of work was done, it is remarkably good, and in all of the papers cited the internal evidence indicates a careful, conscientious, brilliant investigator. There can be no doubt that the disease is due to a bacterial parasite; but to complete the proof that the disease is due to the specified organism it should be obtained by infections with pure cultures obtained from single colonies. The organism thus isolated should also be studied under a wider range of artificial conditions than were employed. Indeed, excluding the pathogenic test, it is more than doubtful if the organism could be identified from the description.

EDITOR'S TABLE.

The rules of zoölogical nomenclature formulated by Strickland and adopted by the British Association for the Advancement of Science, in 1842 have been observed by most zoölogists ever since. They are eminently fair, and conducive to the best interests of science, and in broad contrast in certain details to some individual opinions which have been promulgated in recent years.

There is a minor point in which it seems to us that the Stricklandian rule might be amended, and we recommend it to the consideration of the international zoological congress committee on nomenclature. This is the question of the presence or absence of the annectant i in the root of proper names of the second declension—to which most proper names belong. Shall we write Boggsus or Boggsius: Keenus or Keenius; Levius or Leviius, etc.? The British rule provides (Proceeds. Brit. Ass. Adv. Sci., 1842, p. 115) that after a consonant the termination of proper names shall be *us* gen. *i*; while after a vowel the i shall be inserted, so that we have *ius*, gen. *ii*.

This rule, however, does not exactly conform to the usage of the Romans, which was not regular. Thus they wrote Catullus, Catulli, but Sallustius, Sallustii; Corvus, Corvi; Horatius, Horatii, etc. After vowels the custom also varied, but generally the i was omitted since it is unnecessary on the score of euphony. The Romans were, as well known, guided by euphony in the matter, hence the irregularity. It is evident that we should be guided by the same principle, but that in doing so we should endeavor to formulate a rule which shall have no exceptions. Naturalists cannot be expected to remember exceptions in a subsidiary matter like nomenclature.

The reversal of the Stricklandian rule would apparently accord best with the spirit of Latin word composition. That is, an i should be inserted after the root of all proper names of the second declension which ends in a consonant, and no i should be inserted where the root terminates in a vowel. Names of the first class never sound badly with the i, while most of them,—notably those whose roots end in labials and dentals, do sound badly. A vowel precedes the us euphoniously. Thus Dana, Danaus; Perrine, Perrinius; Secchi, Secchius; Gaudry, Gaudrius. Those ending in o and u are not of the second declension, unless made so by the addition of the consonant v, as Sello, Sellovius; Yarrow, Yarrovius.

Our much esteemed contemporary, *Natural Science*, had, in a recent number, three short articles devoted to the denunciation of the describing of species in biology; calling the practice in one of them "a most unprofitable" kind of work. Now comes our equally esteemed colleague, *The Revue Scientifique* (1896, p. 440), and remarks as follows, anent of the recent work of Messrs. C. H. Merriam and E. S. Miller on North American Mammalia: "But really is there not more interesting work to be done on the fauna of the United States? This work, which consists in enumerating and describing species, which is within reach of the most mediocre intelligence, this fastidious care which should be left to those who are not capable of ideas, is this the only work which tempts American Zoologists? Is there not other occupation for their scientific activity? Cannot Mr. Hart Merriam stimulate work of a biologic character?"

We regard the expressions above quoted as an indication of a mild form of megalomania which is not unfrequently found among the users of mechanical appliances in the biological laboratory. The most intelligent cultivators of these important branches of biologic research are, however, well aware that the exact determination of species is fully equal in importance to their own pursuit, for the following reasons, among others. If we regard biology to consist of two branches, evolution and physiology, we define evolution, with Darwin, as the origin of *species*. For physiology the question of species is not so important. Species are, however, what the labors of the ages have produced, and it is necessary to know them in order to pursue any branch of evolution (as embryology or paleontology) intelligently. The work of the embryologist and paleontologist who does not know the species whose origin he seeks to explain is greatly lacking in precision. Linnæus states that the tyro knows the higher divisions, but only the expert knows species. We also especially deny that the discrimination and description of species is within reach of the most mediocre intelligence. On the contrary, no kind of work in biology imposes as much on all the mental faculties which are used in scientific work. Those who have not attempted it have little idea what is involved in a diagnosis or an analytical key. Finally, as regards the mammalogic work of Messrs. Merriam and Miller, we consider it of the utmost importance. They are pointing out the results of the evolution of Mammalian life in North America, which it is the business of the embryologist and the paleontologist to explain. And in this field the work of Messrs. Merriam and Miller is the best that has ever been done in any country.

The most important result of the Nansen Arctic exploration which has been so far given to the public is the discovery that the ocean has the great depth of nearly 2000 fathoms north of Franz Joseph's Land. This is the average of the oceanic depths, and the knowledge of its extension to the point nearest the pole yet attained, is a distinct gain. It dispels the idea that the pole can be reached overland from the side of Siberia, and shows that the nearest land approach, as suggested by Peary, is by way of Greenland. While this discovery does not destroy the hypothesis that land exists near the pole, it weakens it. The theory will not become extinct until the northern rendezvous of high arctic migratory birds has been discovered. The remarkable discovery of a territory free from glaciers and covered with vegetation in Grinnell Land, and along the north coast of Greenland, by the Greeley Expedition, opens up interesting possibilities, and must stimulate further search. American citizens have had an honorable share in these in the past, and it is to be hoped that they will continue to attack the problem until it is solved.

RECENT LITERATURE.

The Earth and Its Story by A. Heilprin[1] fills a want long felt by teachers of elementary geology. It is a well illustrated little volume which presents "briefly, forcibly and possibly in a more popular form than in most books of a similar nature, the general facts of geology." It covers the field that it is intended to cover in a remarkably satisfactory manner. The facts of the science are given in sufficient detail to impress the student with the notion that the generalizations based upon them are built upon a secure foundation. Comparatively slight stress is laid upon these facts, the greater emphasis being placed on the general truths to which they lead. The book is interesting. It is well written; the language is simple and the thoughts are very clearly expressed. Only the most important conclusions of geology are mentioned, and where the views expressed are not accepted by all geologists, the author does not hesitate to mention the fact.

A prominent feature of the book are the illustrations. These are mainly reproductions of photographs, many of them entirely new. A

[1] Angelo Heilprin: The Earth and Its Story, a First Book of Geology. Boston, Silver, Burdett and Co., 1896. Pp. 267 and Plates 64.

few are blurred, but the majority are sufficiently full of detail to be of great aid to the reader. Two might well have been spared without injuring the value of the volume in the least—the map of Mammoth Cave (Plate 22, Fig. 2) on which the lettering is so small as to be read with difficulty, and the plate supposed to show the forms of crystals.

Criticism might well be urged against the table of geological "epochs and formations," since the terms 'primary' and 'secondary' are used in conjunction with Paleozoic and Mesozoic, as though they were in as frequent use as the latter, and the term 'tertiary' is used as synonymous with Cainozoic. 'Azoic' is also used as the time term corresponding to the formation term Archean, in spite of the fact that the presence of fossils in the Archean rocks (Huronian and Laurentian) is not positively denied. Finally the term Algonkian has no place in the table. While, of course, it is permitted to the author to decline to accept this term as having a definite significance, it is at the same time unfortunate for his readers that they are not made familiar with it, if only as an aid toward the understanding of the handsome geological maps of the U. S. Geological Survey.

There are 19 chapters in the book. The first three treat of rocks, their formation and decay, the fourth of mountains, the next two of glaciers, the seventh of underground waters, the eighth of the relation between sea and land, the ninth of the interior of the earth, the tenth and eleventh of volcanoes, the twelfth of coral islands, the next three of fossils—their organization and their teachings, the sixteenth of land surfaces, and the last three of metals, minerals, building stones, etc.

No one need hesitate for an instant in recommending this little volume for use in our high schools and academies. It is by far the best thing of its kind that has yet appeared upon the market.—W. S. B.

A Handbook of Rocks, for use without the Microscope by Dr. J. F. Kemp[2] is a very welcome visitor to the desk of the teacher of geology. There has long been needed a little treatise on lithology which might be used as an introduction to the study of rocks and as a text-book for the use of those students in geology who have no intention of taking up the subject as a specialty. The volume before us fills this need completely. It is an excellent little book, as full of detail as is desirable for a book of its character and as accurate as is possible in one of its size. Each of the main families of rocks is well characterized

[2] J. F. Kemp: A Handbook of Rocks, for use without the Microscope with a glossary of names of Rocks and other Lithological Terms. Printed for the author. New York, 1896, pp. vii, 176. Price in lots of ten copies $1.00 each.

in a few discriminating sentences, analyses of many varieties are given and the structures and textures of all are well described. One of the most commendable features of the volume is the use of only the more important rock-names in the body of the text—the less important ones being relegated to a very comprehensive glossary which forms a convenient appendix to the book. In this respect, as in some others, the volume under review is very much more satisfactory to the untechnical reader than the other volumes of similar character that have recently come under our notice.

The work opens with a description of the rock-forming minerals and a discussion of the principles of rock classification. Following this are the descriptions of the rocks. These are divided into Igneous, Aqueous (including Eolian) and Metamorphic rocks. Each class is divided into groups according to chemical composition, and each group is further subdivided according to texture. The classification is an eminently practical one, and at the same time it can give no offense to the micoscopical lithologist.

In the discussion of the rock-types each chapter begins with a list of analyses; this is followed by comments upon them. Then comes a description of varieties, a statement of relationships, a paragraph on geological occurrence, one on alterations and one on distribution. In that portion of the book that deals with the igneous rocks the glasses are first taken up, then the porphyritic varieties and, finally, the granitic ones. The aqueous rocks are grouped as mechanical sediments, limestones, organic remains and precipitates from solution. Of the metamorphic rocks two great classes are recognized, viz., those produced by contact action and those produced by regional metamorphism.

The above outline of the contents of the volume is very brief, but it is sufficiently full to indicate that the author has covered well the field that such a treatise as this one should cover. This book should find a wide sale among engineers as well as among all teachers who introduce into their courses on geology a description of rocks. It is a far more valuable synopsis of the characteristics of rock types to place in the hands of geological students than the synopses contained in the large text books on geology.—W. S. B.

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General Notes.

MINERALOGY AND CRYSTALLOGRAPHY.[1]

Etched Figures on Some Minerals.—Traube[2] brings into deserved prominence the value of the method of etching, and gives the results of an extended series of experiments on the etched figures of a number of minerals. He mentions especially those cases in which the etched figures indicate a higher symmetry than that occasionally shown by the geometrical development of the crystal form. He evidently lays more stress on the etched figures of crystals than on the occasional growth of planes corresponding with a lower symmetry. K F and K F, H F are mentioned as giving good results in many cases where the problem is to etch one of the more refractory silicates, and a caution is given that care must be taken in the use of such powerful reagents.

On cuprite etched figures were produced by H_2SO_4, HCl, HNO_3 and KOH, dilute HNO_3 giving the sharpest figures. The etching indicates a holohedral regular symmetry, notwithstanding that Miers has observed faces of the form (986) in a position suggesting gyroidal hemihedrism.

Phosgenite gives sharp figures with hydrochloric, sulphuric, nitric and acetic acids, also with the caustic alkalies, all pointing toward holo-

[1] Edited by Prof. A. C. Gill, Cornell University, Ithaca, N. Y.

[2] Neues Jahrb. B. B. X, pp. 454–469, 1896.

hedral symmetry. The forms developed on some crystals from Monte Poni had suggested trapezohedral hemihedrism in the tetragonal system.

Wulfenite from several localities has been reported to be hemimorphic, on the strength of the polar development of the crystal form, but neither the etched figures nor the pyroelectric behavior of the crystals bears this out. Both wulfenite and scheelite act alike in these latter respects and appear to be pyramidal hemihedral, without difference in the two directions of the vertical axis.

Chalcolite, disthene, tourmaline, vesuvianite, dioptase, willemite, nepheline, beryl, adularia and some of the triclinic feldspars were also etched, with the result of confirming the higher symmetry in each case where doubt could exist. Nepheline, as already established by Baumhauer, belongs to the pyramidal hexagonal class of Groth (1st hemimorphic tetartohedral division of the hexagonal system, Liebisch).

Pollucite, Mangano-columbite and Microlite from Rumford, Maine.—These minerals were discovered in pegmatite associated with quartz, feldspar, muscovite, tourmaline, lepidolite, spodumene, amblygonite, beryl, cassiterite and columbite. They are described by H. W. Foote.[3] The pollucite, though rare, occurs in rather large masses difficultly distinguishable from white quartz. The analysis proves the mineral to be chemically identical with that from Hebron, Maine, and seems to sustain the view of Wells that the formula is $H_2Cs_4\ Al_4(SiO_3)_9$.

The Mangano-tantalite is in the torm of dark reddish-brown crystals resembling rutile. A qualitative analysis revealed the presence of Mn, Ta and Ni. The specific gravity, 6.44, would indicate that the last two elements are present in about equal proportions. The form differs somewhat from columbite, as shown among other facts adduced, by the axial ratios.

	Columbite.	Mangano-columbite.
a : b : c =	.8285 : 1 : .8898	.8359 : 1 : .8817

Microlite in beautiful honey-yellow crystals 2 mm. in diameter have a specific gravity of 5.17. The prevailing form is the octahedron, modified by the dodecahedron and sometimes by (113).

Epidote and its Optical Properties.—The peculiar appearance of a gray epidote from Huntington, Mass., led to its detailed investigation by Forbes.[4] The light color is evidently due to the low percent-

[3] Am. Jour. Sci., CLI, pp. 457–461, June, 1896.

[4] Am. Jour. Sci., CLI, pp. 26–30, 1896.

age of iron, as shown by the subjoined mean of two closely agreeing analyses.

SiO_2	37.99
Al_2O_3	29.53
Fe_2O_3	5.67
FeO	.53
MnO	.21
CaO	23.85
H_2O	2.04
	99.82

This corresponds with the accepted epidote formula. Some of the angles vary quite considerably from those given by Kokscharow—possibly due in one or two cases to the striated character of the faces.

The optical properties are unusual. The axis of greatest optical elasticity lies in the *obtuse* angle β, making an angle of 1° 51′ to 2° 47′ with the vertical axis, according to the nature of the light used. The optical sign is positive—an unusual thing for epidote. The indices are $\alpha = 1.714$, $\beta = 1.716$, and $\gamma = 1.724$. The double refraction is thus .010, the least value known for the mineral. The optical angle over a, 90° 32′, is exceptionally large. A comparison of the data at hand seems to show that with increasing percentage of iron the double refraction becomes stronger, the index of refraction increases, while the optical angle (over a) grows larger, and when it passes 90° the crystals become optically negative.

Miscellaneous Notes.—Leiss[5] gives details concerning several new models of optical instruments as manufactured by Fuess of Steglitz, near Berlin. The most important of these are a petrographical microscope, a theodolite-goniometer, an optical angle instrument, and a number of devices for universal motion.—Viola[6] shows the application of the quaternion method to the discussion of crystal symmetry, and arrives at results concordant with those of Fedorow, Schönflies and others.—Schwarzmann[7] describes a scale for reading directly with approximate accuracy the apparent optical angle 2E, without the labor of calculating it by Mallard's formula.—Crystallographers will be much interested in the results obtained by Rinne[8] in certain experi-

[5] Neues Jahrb., B.B. X, pp. 179–195; also pp. 412–439, 1895.
[6] Neues Jahrb., B.B. X, pp. 495–532, 1896.
[7] Neues Jahrb., 1896, Vol. I, pp. 52–56.
[8] Neues Jahrb., 1896, Vol. I, pp. 139–148.

ments on heulandite. Anhydrous H_2SO_4 abstracts 2 H_2O from the molecule Ca Al_2 Si_6O_{16} + 6 H_2O leaving Ca Al_2 Si_6O_{16} + 4 H_2O. The latter compound is optically quite different from the original heulandite, having, for example, a much higher double refraction and a different plane of the optical axes. The change may be watched under a microscope, and takes place faster in some crystallographic directions than in others. Dilute sulphuric or hydrochloric acid gives a pseudomorph, which, after heating, is composed of almost pure SiO_2 (only 1.33 per cent bases). It has a specific gravity of 2.143, is optically biaxial with a small angle, and has weak double refraction. It is regarded as a new modification of SiO_2, probably like Scacchi's "granulin."

In continuation of his studies on Algerian minerals, Gentil[9] mentions with more or less detail calamine, smithsonite, sphalerite, calcite, galena, cerussite, limonite and barite from a number of zinc mines. Ilvaite and bustamite from Cape Boujaroun are also studied somewhat at length.—Dufet[10] publishes the results of a crystallographic study of four modifications of indophenol, also of several complicated organic and inorganic compounds which are not at all related to one another. —Lacroix[11] describes the microscopical characters of a number of compact or earthy minerals. They are not amorphous, as they appear to the naked eye, but are all micaceous and crystalline in ultimate structure.—Termier[12] reports seven new forms, and a large number of rare ones, on a quartz crystal discovered on a block of gneiss in the lateral moraine of the lower Grindelwald glacier. The new forms are

$$7.7.0.4$$
$$32.\bar{1}5.\bar{1}7.62$$
$$\bar{1}1.\bar{2}5.36.0$$
$$10.\bar{3}5.25.20$$
$$\bar{1}.4.5.\bar{2}$$
$$\bar{1}7.\bar{4}.21.\bar{9}$$
$$\bar{3}.\bar{4}.7.\bar{7}$$

The explanation of these rare faces is sought in the deposition of calcite on the quartz, followed by the formation of "temporary limiting" faces as the crystal again grows, and, finally, the solution of the out-

[9] Bull. Soc. Fr. Min., XVIII, pp. 399–414, 1895.
[10] Bull. Soc. Fr. Min., XVIII, pp. 414–426, 1895.
[11] Bull. Soc. Fr. Min., XVIII, pp. 426–430, 1895.
[12] Bull. Soc. Fr. Min., XVIII, pp. 443–457, 1895.

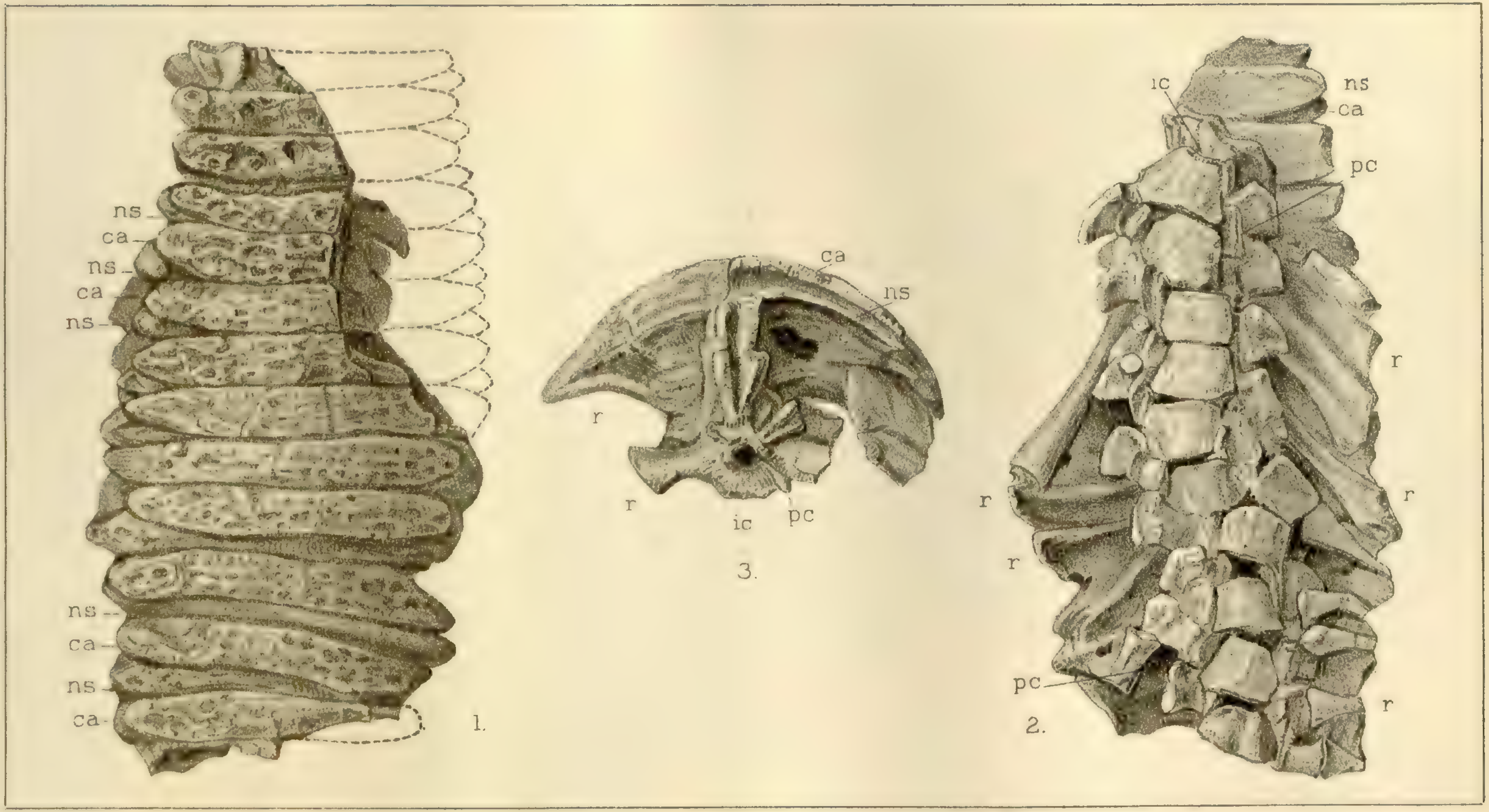
side of the quartz, thus exposing again the unusual faces. Some of the calcite layers are still present in the specimen.

The new mineral lawsonite is more fully described by Ransome and Palache[13] than was the case in the original paper by Ransome. The formula is H_4 Ca Al_2 Si_2O_{10}.—Walker[14] finds that the sperrylite from the Sudbury district probably occurs originally included in chalcopyrite. The new face π (10.5.2) was observed. The suggestion is made that Os and Ir occur replacing Pt in sperrylite, and an analysis of the products of the Murray mine, showing the presence of these elements, is given. (If, as this analysis would indicate, the two elements osmium and iridium are present in an amount equal to one quarter that of the platinum, it is difficult to suppose that they exist in the sperrylite, since Wells states specifically that he found no iridium in the sperrylite analyzed by him).—Adams and Harrington[15] describe a new alkali-hornblende chemically near an orthosilicate, and a titaniferous andradite from the nepheline-syenite from Dungannon, Hastings Co., Ontario.—Merrill[16] notes an occurrence of free gold in a black mica granite from Sonora, Mexico, apparently as an original constituent of the rock.—Crocoite crystals from Mt. Dundas, on the west coast of Tasmania, measured and figured by Palache[17] present, in addition to the twelve known forms the new, though doubtful, prism (10.3.0).

GEOLOGY AND PALEONTOLOGY.

Permian Land Vertebrata with Carapaces.—In the NATURALIST for 1895 (November) I described under the name of Dissorhophus a new genus of probably Ganocephalous Stegocephalia with an armadillo-like carapace. In the Proceedings of the American Philosophical Society for the same year and month I described a new family of Cotylosaurian Reptiles protected by a similar structure. These constitute the only forms of land vertebrates so constructed known from the paleozoic formations. The nearest approach to it previously known from the Permian is seen in the genus Zatrachys, where the

[13] Zeitschr. f. Kryst., XXV, pp. 531–537, 1895.
[14] Zeitschr. f. Kryst., XXV, pp. 561–564, 1895.
[15] Amer. Jour. Sci., CLI, pp. 210–218, 1896.
[16] Am. Jour. Sci., CLI, pp. 309–311, 1896.
[17] Am. Jour. Sci., CLI, pp. 389–390, 1896.



DISSORHOPHUS ARTICULATUS COPE 5/6

1.

1. OTOCOELUS MIMETICUS COPE 3/5
2. O. TESTUDINEUS COPE 2/3

species *Z. apicalis* has the apex of the spines dilated and sculptured on the superior or external surface, indicating the presence of a row of osseous shields covered by epidermis only, extending along the middle dorsal line. In the Trias, two such types have been previously known; viz., the genus Typothorax Cope, from New Mexico, and Aëtosaurus Fraas from Würtemberg.

The discovery of the Permian form in question is important from various points of view. The discovery confirms again a hypothesis proposed by me, several years previously (NATURALIST, 1885, p. 247, Transac. Amer. Philos. Soc., 1892, p. 24). It presents us with what had been previously wanting, forms ancestral not only to the Triassic Reptilia above referred to, but also ancestral to the order of the Testudinata, which according to Quenstadt and Baur appears first in the Trias. The discovery also brings to light an interesting case of homoplassy, since we have two families in no way allied to each other, the one a Batrachian, and the other a reptile, presenting an identical character, and which is so closely similar in the two, that the carapaces cannot be well distinguished on an external view. Internally, however, the characters differ widely. In the case of the Reptilian family (Otocœlidæ) the structure is what one finds in the Testudinata and Pseudosuchia (Typothorax); while in the Batrachian it is constructed by an expansion of characters already known in other Stegocephalia.

For the accompanying illustrations I am indebted to the American Philosophical Society.

PLATE XXI.

Otocœlus testudineus Cope, From above x.66.

PLATE XXII.

Dissorhophus articulatus Cope, x.82; 1 above; 2 below; 3 anterior view.—E. D. COPE.

Ameghino on the Evolution of Mammalian Teeth.[1]—The discoveries of M. Ameghino in Argentina have put him in a position to throw a great deal of light on the evolution of the Mammalia. Several problems which are presented by general Mammalian dentition should be greatly elucidated by his material, and some of those suggested by the Toxodont and Edentate types are within his reach almost to the exclusion of other investigators. He has already made important con-

[1] See l'Evolution des Dents des Mammiferes par Florentino Ameghino. From the Bull. Acad de Ciencias de Cordoba, XIV, p. 381; Buenos Ayres, 1896.

tributions to the histories of both these orders, while other problems remain open.

In the paper of about 1060 pages now before us, M. Ameghino gives his views on the general subject. It seems that in his work Filogenia, published in 1884, he adopted the view of Gaudry of 1878, (previously barely suggested by others), that complex teeth of Mammalia are produced by the fusion of a number of originally distinct simple teeth; a view which has been supported by Kükenthal and Röse on embryologic grounds. It had been previously believed that additional cusps are the product of plications of the dental crowns of simple teeth, and in 1873 and later I had constructed on that basis a phylogenetic system of dentition. This, as is well-known, proceeds from the simple to the complex, without the element of fusion entering at any point. The series is, for the upper jaw; the haplodont, triconodont, tritubercular,[2] (sectorial) quadritubercular, quinque- and sextubercular, and the various lophodont forms; for the lower jaw; haplodont, triconodont, tritubercular, tuberculosectorial, (sectorial), quadritubercular, and the various lophodonts. This succession corresponds with the time order both in North America and Europe, and it is to be supposed that it must, therefore, do so in other parts of the earth, wherever the Mammalia have developed a dentition beyond primitive types.

I have never attempted to bring into this system the Monotrematous Prototheria, and have maintained that they constitute a distinct phylum.[3] My discovery that the dentition of the Permian Cotylosaurian family of the Pariotichidæ consists of simple teeth arranged in transverse series,[4] induced me to remark[5] "that the only question that could arise" as to the hypothesis of dental fusion "is with regard to the Multituberculata." A fusion of the teeth of the Pariotichidæ could produce molars like those of the Multituberculata; but there is no evidence that such a fusion has ever occurred.

Returning to the Eutherian Mammalia, we observe that Ameghino believes that the complex molars have preceded the simple ones in the order of time, and that the tritubercular molar is the result of a loss of a tubercle of the quadritubercular; the quadritubercular the result of

[2] Rütimeyer used the term trigonodont for triangular molars, without specification of the number of tubercles. This word cannot take the place of tritubercular, since the evolution is a question of tubercles, and not of shape. Some tritubercular teeth are quadrangular (Periptychus) and vice versa.

[3] See Amer. Journal of Morphology, 1889, p. 146.

[4] Proceeds. Amer. Philos. Society, 1895, 439–444.

[5] Primary Factors of Organic Evolution, 1896, p. 334. AMER. NATURALIST, 1896, Plate VIIa, p. 801.

reduction of a still more complex molar. Most of the evidence for this conclusion is derived from the fact, as he believes, that the Mammalia of Eocene and possibly earlier age, which are found in Argentina generally, have quadritubercular molars. In accordance with this view Cetacea and Edentata with numerous teeth, present a primitive type of dentition which has survived.

The reply which can be made to this fundamental proposition as to time-order, is, that M. Ameghino has probably affixed too great an age to his earlier beds. This is the opinion of Lydekker, and such extinct types as occur in those beds which occur elsewhere confirm this conclusion. Thus the Patagonian, which Ameghino regards as an Eocene formation, containing the Pyrotherium, contains also the primitive monkeys Anthropops, and the cetaceous Prosqualodon, Argyrocetus and Diaphorocetus. Now Diaphorocetus and forms closely allied to Arygocetus and Prosqualodon are characteristic of the middle Miocene in North America and Europe. It is highly improbable that the quadrumanous genera discovered by Ameghino are of Eocene age, since nothing of the kind occurs in Eocene beds in the Northern Hemisphere, where more primitive and ancestral lemuroid families represent them. The presence of supposed Condylarthra (not yet described) however, gives an Eocene character, and if the forms described by Ameghino as Multituberculata are really such, this character would be difficult to deny. However, recently Ameghino has recognized that these forms do do not belong to that order, but are true Marsupialia, and Lydekker assert that they do not belong to the Patagonian formation, but to the overlying Santa Cruz beds.[6] But supposing that the Patagonian formation is upper Eocene, it does not furnish the material for an elucidation of the dental characters of the primitive Mammalia. These are only partly displayed in the lower Eocene, for it is in the Postcretaceous (Puerco and Laramie) that the true ancestral relation of the tritubercular molar is fully seen. These formations may be represented by the lower or dinosaurian beds which lie below the Patagonian formation in Argentina, but no Mammalian remains have been found there thus far by Ameghino. The oldest Mammal is said to be the Pyrotherium of the Patagonian formation, but it has an aspect more modern than Eocene. It is suspected by Ameghino to be a proboscidian, but it has not yet been shown that it is not a marsupial.

Dr. Ameghino misinterprets North American fossils in more than one instance. He cites the Amblypoda in evidence of the proposition that the tritubercular molar is the result of a reduction of the quadri-

[6] Geographical History of Mammals, 1896, 115. Ameghino makes the same statement in Enum. Synopt. Mamm. Foss. Eocene de Patagonie, 1894, p. 10.

tubercular. His series is Uintatherium, Coryphodon, Pantolambda; the last the most completely tritubercular. The time order is, however, the reverse, viz.: Pantolambda (Puerco), Coryphodon (Wasatch, and Uintatherium (Bridger); the first the most unmodified tritubercular.

In accordance with his general position Dr. Ameghino believes (p. 72) that the quadritubercular genus Procyon is of great antiquity and prior to tritubercular types. This, however, cannot be believed. It has descended from a primitive plantigrade tritubercular, canine type, as have their allies the bears. The same modification is seen in the Mustelidæ in the badgers; and all such are modern forms. He states (p. 26) that in Periptychus and Mioclænus, Phenacodus and Achænodon, the teeth are quadritubercular. The first two genera have tritubercular molars with insignificant rudiments of others both before and behind the protocone (Periptychus) or behind only (Mioclænus), and they belong to the primitive Puerco period. The other two genera are quadritubercular, but belong to later beds, Phenacodus being Wasatch, and Achænodon, Bridger, neither of which formations has any genus in common with the Puerco (except Didymictis of Puerco and Wasatch ages).

Dr. Ameghino believes the Typotherian suborder of the Toxodontia to be related to the Quadrumana. The digits resemble decidedly those of that suborder, but one important difference is overlooked by him. He has pointed out the striking alternation of the two rows of carpal bones in the Typotheria, in which they agree with the Toxodontia proper, and with the Amblypoda. Now in primitive Quadrumana this alternation does not exist, but the bones of the two carpal rows, like those of the tarsus, are directly juxtaposed, or taxeopodous. This characterizes the Condylarthra, which furnish the exact foot characters of the lemuroids, or ancestral Quadrumana.

Finally as to Dr. Ameghinos' views of the origin of the Cetacea, he again inverts the order of succession. He does this by assuming that the Archæoceti are not related to the Cetacea proper, and cannot be ancestral to them. He does not regard the presence of two rooted molars in the fœtal Balæna as significant in this direction. The opinion of zoologists and paleontologists has been different from this, and I have confirmed the general view in my recent researches on the extinct Balænidæ of the Eastern United States.[7] I have shown that a decided sagittal crest like that of the Zeuglodontidæ exists in some of the Miocene whalebone whales. In my estimation the simple teeth of many Cetacea are the result of a process of dental degeneration.

[7] Proceeds. Amer. Philos. Soc., 1895, p. 139; 1896, p 141.

Their increase in number has not been due to subdivision of primitive teeth as supposed by Kükenthal, nor is it a survival of primitive conditions, as supposed by Ameghino, but it is probably a repetition of similar structures due to an extension of the dental groove and dental lamina, following the gradual elongation of the maxillary and premaxillary bones, proceeding contemporaneously with degeneracy of the teeth themselves.—E. D. Cope.

Eozoon canadense.—In recent numbers of the *Geological Magazine*, Dr. Dawson cites the evidence to date for the animal nature of Eozoon. Briefly summarized, the facts are these: (1) The rocks of the Grenville series, where the fossil in question occurs most abundantly, belong to a sedimentary formation. (2) They form a great calcareous system comparable with the metamorphosed Paleozoic calcareous beds of organic origin in petrological and chemical character. (3) New material showing more plainly the structure of the canal systems and tubes, evidences a definite plan of macroscopical structure. (4) Late discoveries of Archaeospherinae and other objects supposed to be organic in pre-Cambrian rocks in Canada and in Europe afford, to some extent, corroborative evidence in favor of Eozoon. (Geol. Mag. 1895.)

Thickness of the Coal Measures.—According to Mr. J. C. Branner, the total thickness of the Coal Measures (Pennsylvanian) sediments in Arkansas is greater than that of the sediments of the same age in other parts of the country or of the world. He gives the following table of comparison: Arkansas, 23,780 feet; Nova Scotia, 16,000 feet; Utah and Nevada, 16,650 (?) feet: Indian Territory, 10,000 feet. Mr. Branner finds a reason for the great thickness in the drainage of the Continent during Carboniferous times. "The rocks of this series in Arkansas afford fossil evidence that this part of the Continent was probably not much above tide level. The drainage from near the Catskill Mountains in New York flowed south and west. The eastern limit of the basin was somewhere near the Archean belt from New England to central Alabama. This Appalachian water shed crossed the present channel of the Missouri from Central Alabama to the Ouachita uplift, and the drainage flowed westward through what is now the Arkansas Valley, between the Ozark Island on the north and the Arkansas Island on the south." (Amer. Journ. Sci., Sept., 1896.)

Geological News.—Paleozoic.—An accumulation of fresh material from the Ichthyologic fauna of the Cleveland Shales, Loraine County, Ohio, has enabled Mr. C. R. Eastman to determine the rela-

tions of certain body-plates in the Dinichthyids. These are the median ventral plates of Titanichthys, and a postero-dorso-lateral of Dinichthys. The author further states that "every plate present in the body armor of Coccosteus has its representative in Dinichthys, and that the conditions of overlap and underlap are the same in both forms. (Amer. Journ. Sci., July, 1896.)

CENOZOIC.—Mr. H. W. Fairbanks states that the Lower Cretaceous age is represented in Santa Barbara County, California, by the Knoxville beds, containing the characteristic Aucella fossils. This is the southern point at which the genus Aucella has been found in California. (Bull. Dept. Geol. Univ. Calif., Vol. II, 1896.)

The skull of *Orycteropus gaudryi* (Ant-Bear or Aard-Vark) from the Lower Pliocene of Samos, described by C. W. Andrews, indicates an animal about one-fifth less than the living species. The close resemblance between the fossil and recent forms is remarkable. Dr. Forsyth Major has pointed out that the former distribution of the genus indicates its northern origin, and that it spread into Africa along with the rest of the Pliocene Mammalia with which it has been found. (Proceeds. Zool. Soc. London, 1896.)

In discussing the geographical distribution of the known Castoroid species, Mr. Merriam notes that the American Castorinae seem to reach their maximum development at or before the beginning of Pliocene time, while the culmination of the Eurasia group appears to occur in the Pliocene. This apparent earlier culmination of the American Castorinae, together with the earlier extinction of certain forms in this country, seem to point to an American rather than to the European origin of the family. (Bull. Dept. Geol. Univ. Calif., 1896.)

According to Dr. Shufeldt, Harpagornis, the fossil bird recently found in New Zealand, represents a more or less aquiline type, that might easily have been the common ancestor to a number of genera of existing modern eagles, as, for example, Haliaetus, Aquila and Thalassaetus. A natural scheme of classification would place it between the genera Aquila and Thalassaetus. (Trans. New Zeal. Inst. [1895], 1896.)

ZOOLOGY.

Fishes in isolated pools.—The occurrence of fishes in pools which have no communication with running streams or large bodies of water has been often noticed, and the explanation of their origin and persistence in such places is in some cases not satisfactory.

In collecting during this month (September) in Camden county, New Jersey, I made the following observations. I fished near Winslow, a pool of about twenty-five feet in diameter, and two feet in depth, with a muddy bottom and a few Nymphæas growing in it. It is distant about a quarter of a mile from an insignificant ditch with a little running water, and is surrounded by higher and sandy ground, offering no possible communication with the ditch. A half mile distant and still more inaccessible is a running stream. From this pool I caught large numbers of the following fishes. *Umbra pygmæa, Apomotis obesus* and *Acantharchus pomotis.* The Acantharchi were small, while the others were fully grown.

A quarter of a mile distant from this pool, and at an equal distance from the ditch above mentioned, and not connected with it by any depression of the surface, is another pool of about thirty feet in diameter. The water reaches a depth of three feet in a limited portion of it, and Nymphæas are more numerous, together with Utricularia, etc. Here I obtained the following fishes in considerable numbers. *Umbra pygmæa, Amiurus prosthistius, Esox vermiculatus, Aphododerus sayanus, Apomotis obesus, Mesogonistius chaetodon, Acantharchus pomotis.* Many of these were fully grown. The turtle *Chrysemys picta* was also abundant.

The mud in the first mentioned pool was light colored, and all the fishes were remarkable for the extreme paleness of their tints. The second pool is situated in better soil and its mud contains much decomposing vegetable matter, and is consequently black. The fishes were all deeply pigmented, including the three species found in the other pool, from which they could be distinguished at a glance. The smaller pool was said to have been dried up during the past summer.

Seven additional specimens of the *Amiurus prosthistius* Cope enable me to verify the characters already given (Proceeds. Acad. Nat. Sci., Philada., 1883, 133), from an examination of four from the Batstow River, New Jersey. In five of the new specimens where I counted the anal rays, they number 26. Prof. Jordan has recently attempted to

identify the species with his *A. erebemmus* in The Fishes of North and Middle America, 1896, p. 139. From Jordan's descriptions it is evident that the *A. prosthistius* is nearer to the *A. natalis* than to the *A. erebemmus*. The spines are not elongated as in the former, nor is the head long and narrowed forward, but it is short and wide; it enters the length (without caudal) 3½ times and not four times. The mouth is relatively wider in the *A. prosthistius*, being .66 of the head-length, and not .5 of it as in *A. erebennus*. The inferior barbels are white in the former, while one may suppose they are black in the *A. erebemmus* from Jordan's description. The supraoccipital spine is widely separated from the dorsal spine. In the specimens from Winslow the anal fin is relatively longer than in those from Batstow; in the former it enters the length (without caudal fin) very little over three times (3.2), while in the latter it enters from 3.5 times in one, to 3.66 in two, and 3.85 in another. The length of the anal rays is .66 of that of the head in the Winslow specimens, and .5 of the head in the Batstow specimens. The latter are of larger size.—E. D. Cope.

On the Mud Minnow (Umbra pygmaea) as an air breather.—In the autumn of 1895, I tried to keep a few fishes alive in a small aquarium, viz., a glass jar holding about a gallon. This was filled with well water and some water plants placed in it which grew well. Various fish were placed in it from time to time but all without exception died in less than six hours except a Mud Minnow (*Umbra pygmæa*) This came to the top at frequent intervals, on each occasion emitting bubbles of air and presumably gulping more down, making considerable noise in so doing. On being placed in well aerated water six weeks or more later, this habit ceased.

The other fishes which were placed in the jar, Catfishes, Minnows, Sunfish, and Suckers would come at once to the top gasping for air, and died in an hour or two.

I have placed other of these fish—*Umbra pygmæa*, in well water and they acted the same way, coming to the top at frequent intervals and "bubbling" each time.

I have never found any of these fish dead in dried up pools, though I have carefully looked for them, presumably their ability to use air for respiration saves them.—C. S. Brimley.

The Peritoneal Epithelium in Amphibia.—In a recent study of the peritoneal ephithelium in Amphibia the following points were noted. The species examined were *Necturus maculatus*, *Amblystoma punctatum*, *Desmognathus fusca* and *Diemyctylus viridescens*. All the

specimens of *Necturus*, *Desmognathus* and *Diemyctylus* were taken from January to April and none were examined after spawning. Specimens of *Amblystoma* were studied shortly before and immediately after ovulation, and in August and December. In all the species cilia were found only in the adult female. They occurred constantly upon the hepatic ligament, the ventral wall of the body cavity, the membranes near the mouths of the oviducts and upon the serosa of the liver. In *Amblystoma* cilia were also found upon the mesoarium and the membranes supporting the oviducts. Some of the adult female *Necturi* possessed cilia also upon the cephalic part of the dorsal wall of the body cavity. The ciliated cells occured either singly or in groups. They were most numerous near the mouths of the oviducts. It was found that the direction of the current produced by the cilia was towards and into the mouths of the oviducts. This and the fact that cilia are present upon the peritoneum of the adult female only would seem to strengthen the theory that the ova when set free in the body cavity are propelled by means of cilia into the oviducts.—ISABELLA M. GREEN.

The Penial Structure of the Sauria.—In the Proceedings of the Philadelphia Academy for August and September I have published a paper on this subject, which gives the results of an investigation into the anatomy of the hemipenes of lizards. Very little attention has been given to the subject hitherto, and our knowledge up to 1856[1] is thus summarized by Stannius: "A duplication or bifurcation of each organ is present in *Lacerta* and in *Platydactylus guttatus*. The copulatory organs of the Chamæleonidæ are distinguished by their shortness. In various Varanidæ which have been investigated the internal cavity (external when protruded) has transverse concentric folds. A fissure interrupts these folds so that they are not complete annuli. The extremity is acuminate and expands at the base, forming a kind of glans."

In 1870[2] J. E. Gray describes and figure this organ of *Varanus heraldicus*, giving the best illustration that I know of. In 1886 Wiedersheim (Lehrbuch der Vergl Anat. Wirbelth.) describes and figures this organ in Lacerta. Besides these references I know of nothing later.

As was to have been anticipated, I have found these organs to correspond with the rest of the structure, and to furnish invaluable aids to the determination of affinities among the Sauria. Reference to them

[1] Zootomie der Amphibien, p. 266.
[2] Annals Magaz. Nat. Hist., 1870, VII, p. 283.

cannot be omitted henceforth in cases where the other characters render the question of affinity uncertain.

In the Sauria the male intromittent organ or hemipenis, presents much variety of structure, showing some parallels to the corresponding part in the snakes. It is, however, rarely spinous, as is so generally the case in the Ophidia, the only spinous forms being, so far as I have examined, the American Diploglossinæ and genera allied to *Cophias.* The higher Sauria have the apical parts modified, as in the Ophidia, by the presence of calyculi. Such are characteristic of the Rhiptoglossa and Pachyglossa. The Nyctisaura possess the same feature. The Diploglossa, Helodermatoidea and Thecaglossa have the organ flounced, the flounces often pocketed or repand on the margin. In the Leptoglossa we have laminæ only; in the Tiidæ mostly transverse, and in the Scincidæ mostly longitudinal. In various genera terminal papillæ are present. The organ may be simple or bifurcate or merely bilobate. I have not met with the case so common in the Ophidia, where the sulcus spermaticus is bifurcate and the organ undivided.

The structures of the hemipenis have a constant systematic value. As in the Ophidia, the value differs with the character, but it varies from generic to superfamily in rank.—E. D. Cope.

Food habits of Woodpeckers.—A preliminary report on the food habits of Woodpeckers has been published by F. E. L. Beal, the assistant ornithologist in the U. S. Dept. of Agriculture. The paper is based on the examinations of 679 stomachs of Woodpeckers, representing 7 species—all from the eastern United States. The results of the author's investigations are summarized as follows:

"In reviewing the results of these investigations and comparing one species with another, without losing sight of the fact that comparative good is not necessarily positive good, it appears that of 7 species considered the Downy Woodpecker is the most beneficial. This is due in part to the great number of insects it eats, and in part to the nature of its vegetable food, which is of little value to man. Three-fourths of its food consists of insects, and few of these are useful kinds. Of grain, it eats practically none. The greatest sin we can lay at its door is the dissemination of poison ivy."

"The Hairy Woodpecker probably ranks next to the Downy in point of usefulness. It eats fewer ants, but a relatively larger percentage of beetles and caterpillars. Its grain eating record is trifling; two stomachs taken in September and October contained corn. For fruit, it seeks the forests and swamps, where it finds wild cherries, grapes, and

the berries of dogwood and Virginia Creeper. It eats fewer seeds of the poison ivy and poison sumac than the Downy."

"The Flicker eats a smaller percentage of insects than either the Downy or the Hairy Woodpecker, but if eating ants is to be considered a virtue, then surely this bird must be exalted, for three-fourths of all the insects it eats, comprising nearly half of its whole food, are ants. It is accused of eating corn, but its stomach yielded only a little. Fruit constitutes about one-fourth of its whole fare, but the bird depends upon nature and not upon man to furnish the supply."

"Judged by the results of the stomach examinations of the Downy and Hairy Woodpecker and Flicker it would be hard to find three other species of our common birds with fewer harmful qualities.

The Ectal Relations of the Right and Left Parietal and Paroccipital Fissures.—A preliminary communication upon this subject was made by Dr. B. G. Wilder at the last session of the American Neurological Association in Philadelphia.

The following abstract presents the salient points of the paper:

"The parietal and paroccipital fissures may be either completely separated by an isthmus, or apparently continuous. When so continuous ectally there may still be an ental and concealed vadum or shallow. Disregarding the vadum on the present occasion, the ectal relations of the two fissures may be designated as either *continuity* or *separation*. That continuity occurs more frequently on the left side has been noticed by Ecker, Cunningham and the writer. Hitherto, however, statistics have included unmated cerebrums as well as mates from the same individuals. The following statement is based upon the cerebrums of 58 adults of both sexes and various nationalities and characters. The speaker has examined 48; the other ten have been accurately recorded by Bischoff, Dana, Jensen and Mills."

"The four possible combinations of right and left continuity and separation occurred as follows."

"I. Left continuity and right separation in 27; 46.5 per cent.

II. Right and left continuity in 22; 38 per cent.

III. Right and left separation in 8; 13.8 per cent.

IV. Left separation and right continuity in 1; 1.7 per cent."

"When five groups of persons are recognized the combinations are as follows:

A. In 8 moral and educated persons, combination I, 62.5; II, 25; III, 12.5.

B. In 23 ignorant or unknown I, 56.5; II, 34.8; III, 8.7.

C. In 20 insane, I, 40; II, 35; III, 20; IV, 5.

D. In four murderers, I, 0; II, 75; III, 25.

E. In three negroes, I, 33; II, 67.

So far as these 58 individuals are concerned, the most common combination, viz., left continuity and right separation, is decidedly the rule with the moral and educated, less frequent with the ignorant and unknown, the insane and negroes, and does not occur at all in the murderers. The only instance of the reverse combination (left separation and right continuity) is an insane Swiss woman. The only two known to be left-handed presented the more frequent combination I. (Journ. Comp. Neurol. Cincinnati, Vol. VI, 1896.)

PSYCHOLOGY.[1]

The Nature of Feeling.—A cardinal point of dispute in current psychology is the nature of feeling. The division of simple feeling into pleasure and pain is generally accepted; the question that remains unsettled is the relation of these latter to sensation. Wundt, Lehmann, Marshall and other recent writers, whose views differ in important respects, agree in regarding pleasure-and-pain as a characteristic of sensation (its *Gefühlston*) like quality or intensity. On the other hand there are those who claim that pain (at least) is a separate species of sensation, with a distinct set of nerves and end-organs. Goldscheider at one time believed that he had discovered these pain nerves, but he has recently retracted this claim. Others, again, regard pain as an extreme form or quality of sensation common to the touch, heat and cold senses.

The problem is somewhat complicated by the ambiguity of the word *pain*. In the sense of "physical pain" (*Schmerz*) it may be a species of sensation; while at the same time in the sense of "displeasure" (*Unlust*) it may be regarded as either an "attribute" of sensation or a second element of consciousness. This distinction is maintained by Münsterberg and Baldwin, among others.[2] The ordinary associations of the word *pain* have undoubtedly biased many writers and helped to keep alive the confusion between its two meanings.

[1] Edited by H. C. Warren, Princeton University, Princeton, N. J.

[2] Dr. Nichols in his criticism of Baldwin in the September number of this magazine certainly misapprehends the latter's view on this point. Cf. Mental Development, pp. 483, f.

Prof. Titchener in treating of the subject in his Psychology[3] endeavors to avoid this ambiguity by discarding the terms pleasure and pain, and using *pleasantness* and *unpleasantness* instead. Apart from his terminology, Prof. Titchener's discussion is of special interest from the fact that, although an earnest follower of Wundt in most respects, he recognizes feeling or affection, as a distinct element of consciousness. Wundt reduces all consciousness (aside from the active) to a single element, sensation; Prof. Titchener restricts sensation to the cognitive side of consciousness, and makes affection a distinct and co-ordinate term.

The mind, or consciousness, he says, "not only senses: it *feels*. It not only receives impressions and has sensations: it receives impressions in a certain *way*. . . . Life means the balance of power (more or less effective) in the perpetual conflict of two opposing forces—growth and decay. No impression can be made upon the living body that does not tend in some way to change this balance. . . . It must help either to build up nervous substance or to break it down. The organism is a whole: and what effects it in either of these ways at one part, must affect it as a whole, in all. The conscious processes corresponding to the general bodily processes thus set up by stimuli—processes not confined to definite bodily organs—are termed *affections*. . . . There are only two bodily processes to give rise to affective processes: the building-up process (anabolism) and the breaking-down process (catabolism). We should expect, then, to find no more than two qualities of affection; and introspection tells us that expectation is correct. The anabolic bodily processes correspond to the conscious quality of *pleasantness*, catabolic processes to that of *unpleasantness*."

Prof. Titchener then examines the relation of affection to sensation. "The processes of pleasantness and unpleasantness seem, at least in many cases, to bear a strong resemblance to certain concrete experiences which we have analyzed, provisionally, as complexes of sensations. Thus pleasantness may suggest health, drowsiness, bodily comfort; and unpleasantness pain, discomfort, overtiredness, etc. . . . Now there can be no doubt of the resemblance in the instances cited. But the reason of it is simply this, that health, drowsiness and bodily comfort *are* pleasant, *i. e.*, that pleasantness is one of the constituent processes, running alongside of various sensation processes, in the total conscious experience which we call 'health,' etc.; and that pain, bodily discomfort and overtiredness *are* unpleasant, *i. e.*, that unpleasantness is one of the processes contained in each of these complex experiences.

[3] An Outline of Psychology, by E. B. Titchener, Chap. V.

Beyond this there is no resemblance: a sensation process is radically different from a pleasantness or an unpleasantness." This difference appears in several ways:

(1). The sensation is looked upon as belonging to the object which gives rise to it, while the affection is regarded as belonging to the subject or conscious self. "Blue seems to belong to the sky; but the pleasantness of the blue is in me. Warmth seems to belong to the burning coals; but the pleasantness of warmth is in me. . . . The distinction is unhesitatingly drawn in popular thought, and clearly shown in language. It points to a real difference between sensation and affection as factors in mental experience—a difference which the psychologist must make explicit in his definition of the two processes. The same difference is observed even when we compare the affective processes with those sensations which are occasioned from within, by a change in the state of the bodily organ. The unpleasantness of toothache is far more personal to me than the pain of it. The pain is 'in the tooth;' the unpleasantness is as wide as consciousness."

(2). If a stimulus be long continued, the affection, if it is not of such a character as to pass over into pain, in the end becomes indifferent, while the sensation remains as strong and clear as ever, when the attention is directed to it. "Nervous substance, at the same time that it is very impressionable, is eminently adaptable. The organism adjusts itself to its circumstances—resigns itself, so to say, to their inevitableness. When once adaptation or adjustment to surroundings is complete, the surroundings cease to be taken either pleasantly or unpleasantly; their impressions are simply received, passively and unfeelingly."

(3). "The more closely we attend to a sensation, the clearer does it become, and the longer and more accurately do we remember it. We cannot attend to an affection at all. If we attempt to do so, the pleasantness or unpleasantness at once eludes us and disappears, and we find ourselves attending to some obtrusive sensation or idea which we had no desire to observe."

(4). "As a general rule, 'central' sensations are much fainter and weaker than 'peripheral.' A remembered noise has hardly anything of the intensity of the noise as heard. Affection can originate in the same two ways. But 'central' pleasantness and unpleasantness are not only as strong as—they are in very many cases stronger than —'peripheral.'"

"We see, then," concludes Prof. Titchener, "that there are strong reasons for regarding affection as different from sensation. It must be

carefully noted that the statements just given of these reasons do not tell us *how* 'red,' a sensation, differs from 'pleasantness,' an affection, in mental experience. They are sufficient indication that a real difference exists; but the difference itself cannot be described—it must be experienced."

It remains to be seen how this theory, or rather Prof. Titchener's restatement of it, will be met by the adherents of the Wundtian view. As to the verbal innovation, the terms *pleasantness* and *unpleasantness* would be more welcome if the proposed meanings accorded better with ordinary usage. Both words, especially the second, are suggestive of a very mild form of feeling; and until we became accustomed to the change it would excite our sense of the ludicrous to call the feeling connected with a violent toothache or an intense abdominal pain *unpleasant*.—H. C. WARREN.

Further Comments on Prof. Baldwin's "New Factor in Evolution."—In a "Note" in THE AMERICAN NATURALIST, October, 1896, Prof. Baldwin declares that I have grossly misunderstood his views, and that, to quote his words, "Dr. Nichols' home thrusts are all directed at my view of pleasure and pain, which he considers, quite mistakenly, the point of my paper. On the contrary, the 'factor' is entirely the influence of the individuals adaptation on the course of evolution; not at all the particular way in which the individual makes its adaptation."

This quotation is typical of the author's style of thinking and writing; of which his critics unanimously complain. The word "influence" is frequently misused by careless writers, as in the above, to denote the *results* of a factor, rather than the factor itself. A "factor" is a set of influences or circumstances contributing *to produce* a result. It is true that an author, if of expansive mind, may run ahead of his subject. It is true, as Prof. Baldwin above declares, that his mind was chiefly on the results supposed by him to be worked by his factor. But he should not forget that he declared himself, in his title, to be writing about his "new factor"; and it was quite correct that he should write about it, since one ought, in Science, to establish the existence of a thing before discussing its effects. It was this last I had in view when, in my paper, I directed my discussion toward demonstrating that his new factor, as specificly described by Prof. Baldwin, was a myth.

I directed my discussion against Prof. Baldwin's views of pleasure and pain because he completely identified his "factor" with his particular and all-expansive views of pleasure and pain. On p. 451 of his

pamphlet, he sums up his June paper in these words: "It seems proper, therefore, to call the influence of Organic Selection "a new factor; . . . . *The ontogenetic adaptations are really new, not performed; and they are really reproduced in succeeding generations, although not physically inherited.*" Here the author correctly, though in flat contradiction to his Note, declares in so many words his factor *to be* Organic Selection, and "ontogenetic adaptations" is for it but another name. Of this fact the words which he italicized leave no doubt. Naturally, to find out most accurately what Prof. Baldwin means by Organic Selection, we go to that part of his writing which most professedly expounds it. This is done in Part IV, p. 541, under the caption: "The Process of Organic Selection." After preliminary remarks, which I shall speak of later, Prof. Baldwin's exposition is in the following words:

"There is a fact of physiology which, taken together with the facts of psychology, serves to indicate the method of adaptations or accommodations of the individual organism. The general fact is that the organism concentrates its energies upon the locality stimulated, for the continuation of the conditions, movements, stimulations, which are vitally beneficial, and for the cessation of the conditions, movements, stimulations, which are vitally depressing and harmful. In the case of beneficial conditions we find a general *increase of movements, an excess discharge of the energies of movement in the channels already open and habitual; and with this as the psychological side, pleasurable consciousness and attention.* . . . This form of concentration of energy . . . is called the "circular reaction." It is the selective property which Romanes pointed out as characterizing and differentiating life. It characterizes the responses of the organism, however low in the scale, to all stimulations—even those of a mechanical and chemical nature— . . . Now, as soon as we ask how the stimulations of the environment can produce new adaptive movements, we have the answer of Spencer and Bain—an answer directly confirmed, I think, without question, by the study both of the child and of the adult, *i. e.*, by the selection of fit movements from excessively produced movements, that is, from *movement variations*. So, granting this, we now have the further question: How do these movement variations come to be produced when and where they are needed?"

Having reduced his problem of "the selection of fit movements," *i. e.*, of Organic Selection, to this pointed inquiry, Prof. Baldwin then proceeds to state his still more explicit exposition of his selective "factor" in full, as follows:

"But, as soon as we inquire more closely into the actual working of pleasure and pain reactions, we find an answer suggested [an answer to the last above quoted question]. The pleasure or pain produced by a stimulus—and by a movement also, for the utility of a movement is always that it secures stimulation of this sort or that—does not lead to diffused, neutral, and characterless movements, as Spencer and Bain suppose; this

is disputed no less by the infants movements than by the actions of unicellular creatures. There are characteristic differences in vital movements wherever we find them. There is a characteristic antithesis in vital movements always. Healthy, overflowing, overstretching, expansive, vital effects are associated with pleasure; and the contrary, the withdrawing, depressive, contractive, decreasing, vital effects are associated with pain. This is exactly the state of things which the theory of the selection of movements from over-produced movements requires, *i. e.*, that increased vitality, represented by pleasure, should give the excess movements, from which new adaptations are selected; and that decreased vitality, represented by pain, should do the reverse, *i. e.*, draw off energy and suppress movement.

"If, therefore, we say that here is a type of reaction which all vitality shows we may give it a general descriptive name, *i. e.*, the "Circular Reaction," in that its significance for evolution is that it is not a random response in movement to all stimulations alike, but that (it distinguishes etc.) it distinguishes in its very form and amount between stimulations which are vitally good and those which are vitally bad, tending to retain the good stimulations and to draw away from and so suppress the bad. . . . *This kind of selection, since it requires the direct co-operation of the organism itself, I have called Organic Selection.*"

"This" (note the last sentence), then, is the "Organic Selection" which Prof. Baldwin himself specifically declares (p. 451) he names a "new factor." As the reader must see for himself, the author's description of it is a description of pleasure-pain functions pure and simple and nothing more. It is not *merely* the old pleasure-pain tradition, for nothing remains inexpansive in this vigorous author's hands. But it is the orthodox tradition unfolded to "a type of reaction which all vitality shows;" which "distinguishes in form and amount between stimulations which are vitally good and those which are vitally bad;" "which is a characteristic antithesis in vital movements always;" which "is the selective property which Romanes pointed out as characterizing and differentiating life;" and which performs its task of the "*selection of fit movements*" generally, by its universal exercise in all creatures from first to last and at all times.

It is dangerous to grapple with an author who is so macrocosmic in his thought, and so amorphous in his diction. But I discussed Mr. Baldwin's "New Factor" from the point of view of his "expanded" pleasure-pain functions because he so completely *identified* it with them. I cannot conceive this to have been done more explicitly and completely than in the author's specific exposition of Organic Selection in his Part IV. Under this situation it was surely "to the point" to prove Mr. Baldwin's New Factor a myth. The tone of Mr. Baldwin's "Note" seems to indicate that this was done with peculiarly exhaustive effect.

A word remains to be said about Mr. Baldwin's complaint that his pamphlet distinctly insisted on *the fact* of Organic Selection, without regard to any "particular way" it may be accomplished. Prof. Baldwin did file such a caveat upon all possible ways which man may ever invent for proving that Organic Selection *may be* a fact. But this is not the method of Science. She does not feel called upon *to invent* all possible ways before she rejects the sole one offered. When Prof. Baldwin does give us some other "particular way" than the one he did give for the operation of his factor, I will, perhaps, then be able to show him it cannot be called "new" with any sort of justice to Darwin and to biologists commonly.

Of the personal tone of Mr. Baldwin's "Note" I have nothing to remark, save by way of gratification, that it is unmatched in American Science.

HERBERT NICHOLS.

Boston, Oct. 14, 1896.

ANTHROPOLOGY.[1]

Pictured Caves in Australia.—In West Australia, New South Wales, Queensland, and doubtless in other parts of Australia, where the geology is favorable, rock shelters and caves have been recently noticed, whose walls are decorated with native allegorical designs and figures of men, birds and animals outlined in colour. Mr. T. Wornsop addressing the Australasian Association for the Advancement of Science at Brisbane in January, 1895 refers to a great number of rock paintings of Kangaroos, Lizards, Emus, Flying birds, Snakes and other forms. Refering to discoveries of these strange and impressively decorated shelters by Sir George R. Grey, Mr. Stockdale, Mr. O. Donnell and others, he states that a general similarity characterizes the designs wherever found, and describes further the curious method of painting generally noted, which appears to consist in smearing the rock surfaces with animal fat, pressing the object to be represented against the greased rock, and then blowing dry color against it so as to thus stencil the outlined form by a surrounding area of contrasting tint. When wet color was splashed on, no grease would have been needed. Mr. W. J. Enright, the discoverer of numerous painted caves and Mr. R. H. Matthews describe in particular the abundant figures of human hands with

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

out stretched fingers apparently painted and stenciled in this manner, often in red, in nearly all the caves. Along the Glen Lake river valley near Kimberly, West Australia and on Bulgar Creek, New South Wales the caves display hearts, white human figures on black backgrounds, staring faces outlined in red, with yellow lines, figures of the rising-sun, and Phallic symbols, where the stenciling according to Mr. Enright has often been done by blowing powered pipe clay from a deposit near at hand (sometimes white and sometimes stained yellow by oxides) upon the greased rock. Strangest sight of all must be the weird shelter on Nardo Creek in Central Queensland where a diabolical picture 70 feet long seems to represent a lake out of which are stretched hundreds of brown human arms pointing, grasping and knotted in many positions as if writhing in torture.

Mr. Wornsop and others looking in vain for a clue to the meaning of the rock paintings, have set in evidence the refusal of neighboring natives to account for them, just as earlier observers in America, were wont to quote indian ignorance of mounds, and earthworks. But on the other hand Mr. Enright noting the fresh appearance of many of the designs, speaks of one of the decorated caves recently inhabited by a native named Cutta Muttan, without doubting that the later had done the painting. No doubt he did, and small question that natives now living in Australia could if sympathetically approached by Ethnologists (who living with them had gained their confidence), explain all the designs. —H. C. MERCER.

Man and the Fossil Horse in Central France.—Not many hundred yards from the classic rock shelters of Laugerie Haute and Laugerie Basse (which contain according to the French classification *Magdalenian* and *Solutrean* culture layers) a recently exposed talus, along the Manaurie brook an affluent of the Vezere (department Dordogne Commune Tayac, France) has revealed an interesting and surprising deposit of human remains associated with bones of the fossil horse. M. M. Chauvet and Riviere digging a trench 17 meters long, 1.80 meters broad and 3 meters deep, found in one day, three hundred and more horse-teeth together with other horse bones generally broken by human hands, besides the remains of the badger (*Meles taxus*) and the canine tooth of a large carnivore. No fresh water or marine shells were found but with the bones about two hundred chipped flint axes ("Turtle-backs") of so-called Chellean type, or of similar ovate form worked only on one side, were unearthed in a few days, with three Mousterian *racloirs*, four discoidal flints, two Magdalanian flakes, two scrapers,

and some nuclei. But few details as to the stratification or formation of the deposit are given in the account published in Cosmos (Sept. 12, 1896, P. 211) and as nothing is said about hammer-stones, and flint chips, we are left to wonder whether the place represents a palæolithic workshop such as Messrs. Spurrel and W. G. Smith found at Crayford and Caddington, England or not. Meanwhile the excavation which we are told is to be continued, if studied with care and without bias may affect the validity of the French subdivisions (*Chellean Mousterian, Solutrean Magdalenian*) of the Palæolithic period in Central France. Judged by the shape of the flint blades found with the horse bones, M. M. Chauvet and Riviere call the deposit *Chello-Mousterian* while hardly a mile away, we have Laugiere Haute classified as showing *Magdalenian* above *Solutrean* culture layers, with Laugiere Basse, Cro-Magnon and Gorge d'Enfer floored with *Solutrean* only. The rock shelters of Le Moustier (*Moustierian*) and La Madeleine (*Magdalenian*) are not far distant and the question is whether all these different geological epochs supposed to indicate intervals of thousands of years, varying stages of human culture and changes in animal life can be justly established at this remarkable neucleus of ages where one more subdivision is proposed to be added to the list of culture layers represented in an area of a few square miles and based on differences in flint chipping, and variations not universally agreed to, in the sequence of animal life.

Chipped Flint blades from Somali Land.—Mr. H. W. Seton-Karr who presented to the British Association for the Advancement of Science at Ipswich, England in 1895 several heavy ovate blades of chipped flint from Somali Land, has brought more recently from the same region others (referred to in Proceedings of Royal Society, Vol. LX, no. 359, p. 19). Often well worked, considerably patenated, and resembling in shape and make, the drift blades of England and France they appear to have been found not in situ but on ths surface, mostly along water courses where rain or wind had bared them of surrounding earth. No excavations were made to ascertain their position with reference to the surrounding geological strata, and no association appears to have been established with the remains of animals living or extinct. Nothing is said of Hammer-stones or chips that might have testified to the existence of blade workshops at the sites, and nothing as yet save the appearance of the blades (some of which are worked only on one side after the French *Moustier* pattern) has been presented to warrant us in setting back the date of these relics to the date of the similar shapes associated with the Mammoth and Rhinocerus in the Somme Valley.

Cave Hunting in Scotland.—If as we understand no chipped blades of the "Turtleback" or drift character have been gathered in Scotland or northern Europe, if no traces of (Paleolithic) man in Association with the Cave Bear, Woolly Rhinocerus and Mammoth have been discovered in caves or quarries anywhere to the northward of middle England or in Scandinavia North Germany and Russia, if in a word it can be proved that snow and ice precluded human presence or obliterated man's foot-prints in northern Europe at the time when drift men were chipping flint on the banks of the Thames and Somme, then the exploration of caves in any part of this colder European region is of particular scientific interest. Near Oban in Scotland the Mackay, Gas works, Distillery, and MacArthur caves recently explored by Mr. J. Anderson for the Society of Antiquaries of Scotland (see proceedings of the Society, vol. XXIX, 1895, p. 211) showed human rubbish deposits consisting largely of the shells of edible mollusks (Ostrea, Patella, Pecten, Solen, etc.), interbedded in one instance (the Mackay Cave) with a gravel layer apparently caused by a marine inundation. In the latter cave, fairly representing the others, Mr. Anderson found in in the shell rubbish about 150 bone needles and points, seven numerously barbed bone harpoons, sometimes with pierced bases, three pebble hammerstones, a few flint nodules, and several flakes and scrapers together with numerous fish bones and the remains of the common deer, the *Bos longifrons*, boar, the dog and the cat; in other words, the recent fauna of the region. The bones of fifteen human skeletons found apparently near the surface and above the shell and bone refuse in the various caves, according to Mr. Anderson and Sir William Turner, represent a people of the Neolithic or late stone age in Europe, while on the other hand M. Boule (see L. Anthropologie, May and June, 1896, p. 321) citing the gravel bed as evidence of an early flood and comparing the barbed and pierced harpoons with similar harpoons supposed to be of an intermediate age (between Paleolithic and Neolithic) from certain French caves, suggests that the Oban remains form a connecting link between the Paleolithic (Mammoth, Rhinoceros and Reindeer time) and the Neolithic (recent fauna time) of western Europe. When all the results of European archæology are summed up it has been supposed that a hiatus in time unbridged by any intermediate human or animal presence, existed between the earlier and later of these periods, and a link will be added to the archæological chain, if discoveries in French caves or elsewhere satisfactorily fill the supposed gap. But whether the remains from Oban can or cannot be assigned this important intermediate position, further investigation will show. For a time the cave explorer might leave

the southern fields where much collaboration has perplexed the subject, and turn northward. There the coast is clear. There evidence broadening the perspective of the European student, and setting a wide geographical limit to the ancient human record, can be established in unexplored caves, where in a new way the unearthed testimony should show the relation of fossil man to glacial ice and cold.—H. C. MERCER.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

New York Academy of Sciences.—Biological Section, October 12, 1896.—Dr. Bashford Dean and Mr. G. N. Calkins presented preliminary reports upon the results attained at the Columbia University Zoological Laboratory at Port Townsend, Washington. The expedition spent about six weeks in exploring and collecting, and brought home large collections from exceptionally favorable collecting grounds. Dr. Dean spent some time in Monterey, Cal., and brought home collections of eggs and embryos of Chimæra and Bdellostoma.

Dr. J. L. Wortman made a preliminary report upon the American Museum Expedition to the Puerco and Wasatch Beds. He reported finding a connecting link between the close of the Cretaceous and the beginning of the Tertiary. He gave an interesting account of the massive ruins of the so-called cliff-dwellers in the region visited by him. In the Big Horn basin the expedition had remarkable success as well as in the Wind River basin.

Prof. Osborn stated that with the collections made this summer the American Museum could now announce that their Eocene collection was complete, containing all mammals now known in the Eocene; that their collection from the Wasatch bed was the finest in existence, and that from the Wind River basin was complete; the Bridger was represented by all but two or three types; and fine collections have been made in the Uintah.

Mr. W. J. Hornaday made a report of a tour of inspection of foreign zoological gardens, made under the auspices of the New York Zoological Society. He visited fifteen gardens in England and on the continent, studying the features of excellence in each.

Prof. Bristol gave a brief account of the progress at the Marine Biological Laboratory at Wood's Hole, Mass., during the past summer.

Prof. Osborn offered the following resolution on the death of Professor G. Brown Goode, after paying a tribute to his memory:

Resolved, That the members of the Biological Section of the New York Academy of Sciences desire to express their deep sense of loss in the death of Professor G. Brown Goode, of the U. S. National Museum. In common with all naturalists in this country, we have admired his intelligence and highly successful administration of the National Museum as well as his prompt and ready response to the requests and needs of similar institutions throughout the country.

In face of the arduous and exacting duties of his directorship he has held a leading position among American zoologists, and we are indebted to him for a series of invaluable investigations, especially upon the fishes.

Those of us who had the good fortune to know Professor Goode personally, recall his singular charm of character, his genial interest in the work of others, his true scientific spirit. We have thus lost one of our ablest fellow-workers and one of the truest and best of men.

The resolution was adopted unanimously by a rising vote.

CHARLES L. BRISTOL, *Secretary.*

The Academy of Science of St. Louis.—At a meeting of the Academy of Science of St. Louis, held October 19, 1896, Mr. Trelease exhibited living flowers of *Catasetum gnomus*, demonstrating the extreme irritability of their tentacles and the precision with which the pollinia become attached to any object touching either tentacle. Mr. J. B. S. Norton presented a list of the Ustilagineæ of Kansas, together with the result of germinations of about one-half of the entire number. Three persons were elected to active membership.

WILLIAM TRELEASE, *Recording Secretary.*

The Biological Society of Washington.—The following communications were made: C. Hart Merriam, "A New Fir from Arizona;" Frederick V. Coville, "Notice of Britton and Brown's Illustrated Flora of the Northern United States and Canada;" Erwin F. Smith, "A Bacterial Disease of Potatoes, Tomatoes and Eggplants;" B. E. Fernow, "Timber line: Its Aspects and Causes."

FREDERICK A. LUCAS, *Secretary.*

SCIENTIFIC NEWS.

A course of eight free lectures mainly upon Science and Travel has been arranged by the Field Columbian Museum for Saturday afternoons in October and November at the usual hour, 3 o'clock. Most of these lectures will be illustrated by steropticon views. Subjects, Dates and Lecturers: Oct. 3.—"Archeological Explorations in Peru," Dr. G. A. Dorsey, Assistant Curator of Anthropology, Field Columbian Museum. Oct. 10.—"A Trip to Popocatapetal and Ixtaccihuatl," Prof. O. C. Farrington, Curator of Geology, Field Columbian Museum. Oct. 17. —"San Domingo," Mr. G. K. Cherrie, Assistant Curator of Ornithology, Field Columbian Museum. Oct. 24.—"Egypt and what we know of her," Dr. J. H. Breasted, Instruction in Egyptology and Semitics, University of Chicago. Oct. 31.—"The Petroleum Industry," Dr. D. T. Day, Chief of Division of Mineral Resources, U. S. Geological Survey. Nov. 7.—"Alaska and its Inhabitants," Prof. George L. Collie, Beloit College, Wis. Nov. 14.—"The Economic Geology of the Sea," Mr. H. W. Nichols, Curator of Economic Geology, Field Columbian Museum. Nov. 21.—"The Physical Geography of New England," Dr. H. B. Kümmel, Assistant Professor of Physiography, Lewis Institute.

Dr. Ludwig Reh, formerly assistant in the Museum at Saō Paulo, Brazil, has been appointed assistant in the Concilium Bibliographicum at Zürich. With this addition to the working force the Bureau will soon bring its work up to date; and its cards will be sent out more frequently than before.

The annual meeting of the American Psychological Association will be held at Boston, December 29th and 30th, 1896, that place and time having been chosen by the American Society of Naturalists and ratified by the President of the Association.

The Executive Committee of the American Society of Naturalists have decided to hold the next meeting of the Naturalists at Boston and have chosen the Inheritance of Acquired Characters for the theme of discussion.

The next session of the Association of American Anatomists shall be held in Washington City, May, 1897, in conjunction with the other societies of the Congress of American Physicians and Surgeons.

J. H. Maiden has been appointed government botanist and director of the botanical gardens of New South Wales, succeeding Charles Moore who held the position for nearly fifty years.

Mr. F. F. Blackman has been appointed assistant in botany in the University of Cambridge, and Dr. E. Albrecht assistant in the Anatomical Institute of the University of Munich.

The Academy of Natural Sciences of Philadelphia has conferred the Hayden Memorial Geological Award for 1896 on Prof. Giovanni Capellini of the University of Bologna.

Prof. A. N. Kuznetzow has been advanced to the position of ordinary professor of botany and director of the botanical gardens in the University of Dorpat.

The Ninth Annual Winter Meeting of the Geological Society of America will be held in the city of Washington, D. C., on December 29, 30, 31, 1896.

Dr. H. Hanns, Th. Loesener and P. Gräbner have been called as scientific assistants to the botanical museum of the University of Berlin.

Dr. L. Kathariner, of Würzburg, goes to the professorship of zoology and comparative anatomy in the University of Freiburg, Switzerland.

Dr. V. Schiffner has been advanced to the position of professor extraordinarius of botany in the German University of Prague.

Dr. A. Möller, of Idstein, well-known for his studies of South American botany has gone to the Forestry Academy at Eberswald.

The Ministry of Education has conferred the title of professor upon the botanist Dr. Kienitz-Gerloff of Weilberg on the Lahn.

Dr. B. Hofer, of the University of Munich, has been appointed professor of fish culture in the Veterinary school at Munich.

Dr. K. Busz, formerly of Marburg, has gone to the University of Munich as extraordinary professor of mineralogy.

Dr. F. W. K. Müller has been advanced to the position of directors assistant in the Museum of Ethnology in Berlin.

Dr. H. F. Reid, of Johns Hopkins University has been advanced to the position of assistant professor of geophysics.

Prof. K. von Kupffer, of Munich, has been elected corresponding member of the Prussian Academy of Sciences.

Dr. V. A. H. Horsley, professor of histology in University College, London, has been made professor emeritus.

Dr. Standenmaier, of Munich, goes to the Lyceum at Friesing as Professor of Chemistry and Mineralogy.

Dr. J. Lerch, well-known for his studies of the Swiss flora, died at Couvet, March 13th of this year.

Canon A. M. Norman is hereafter to be addressed at Houghton-le-Spring, Co. Durham, England.

Dr. F. Kohl has been advanced to the position of ordinary professor in the University of Marburg.

Dr. A. Hosius, professor of mineralogy in the Academy of Münster, died May 11, aged 71 years.

Dr. R. Zuber is now professor extraordinarius of geology in the University of Lemberg.

Dr. A. Zimmermann, of Berlin, goes to the Botanical Gardens at Buitenzorg, Java.

Dr. H. Henking is now professor of zoology in the University of Göttingen.

Prof. W. Tief, of Villach, Carniola, a student of the Diptera, is dead.

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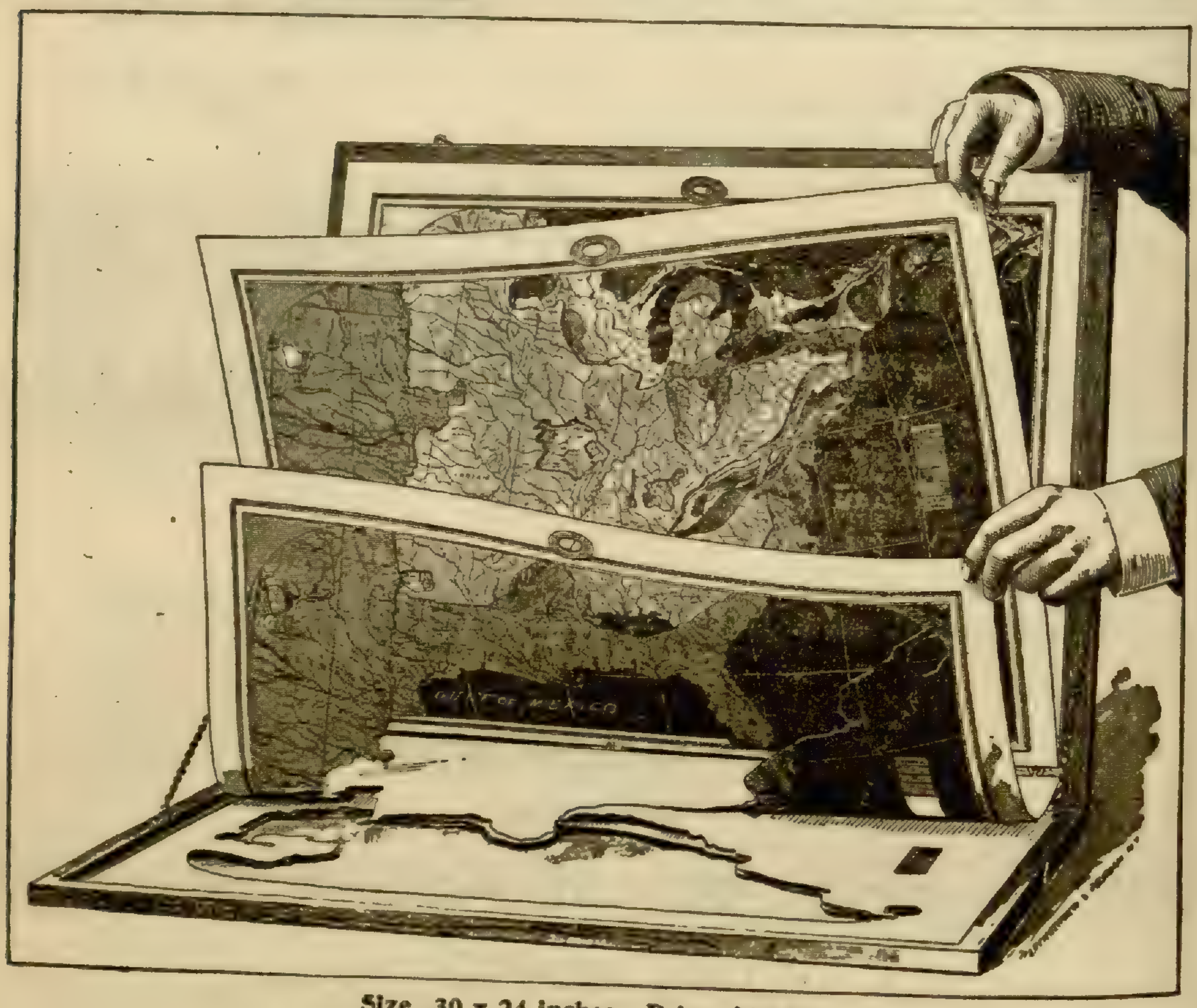

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The following may be mentioned as having contributed to the Volume for '94.

Dr. D. G. Brinton, Rev. Wm. M. Beauchamp, Prof. A. F. Chamberlain, Mr. James Deans, G. O. Dorsey, Dr. J. Walter Fewkes, H. C. Mercer, Mrs. Zelia Nuttall, C. Staniland Wake, Dr. Wm. Wallace Tooker, Dr. Cyrus Thomas. The Magazine during '95 will embrace different departments, and the following gentlemen will have charge and report all explorations and discoveries:

Rev. Wm. C. Winslow, D. D., L. L. D., Egypt.
Prof. T. F. Wright, Explorations in Palestine.
Henry W. Haynes, Paleolithics and European Archaeology.
Dr. A. S. Gatschett, Indian Linguistics.
Marshall H. Seville, Mexico and Central America.
Hon. James Wickersham, The North West Coast and Eastern Asia.

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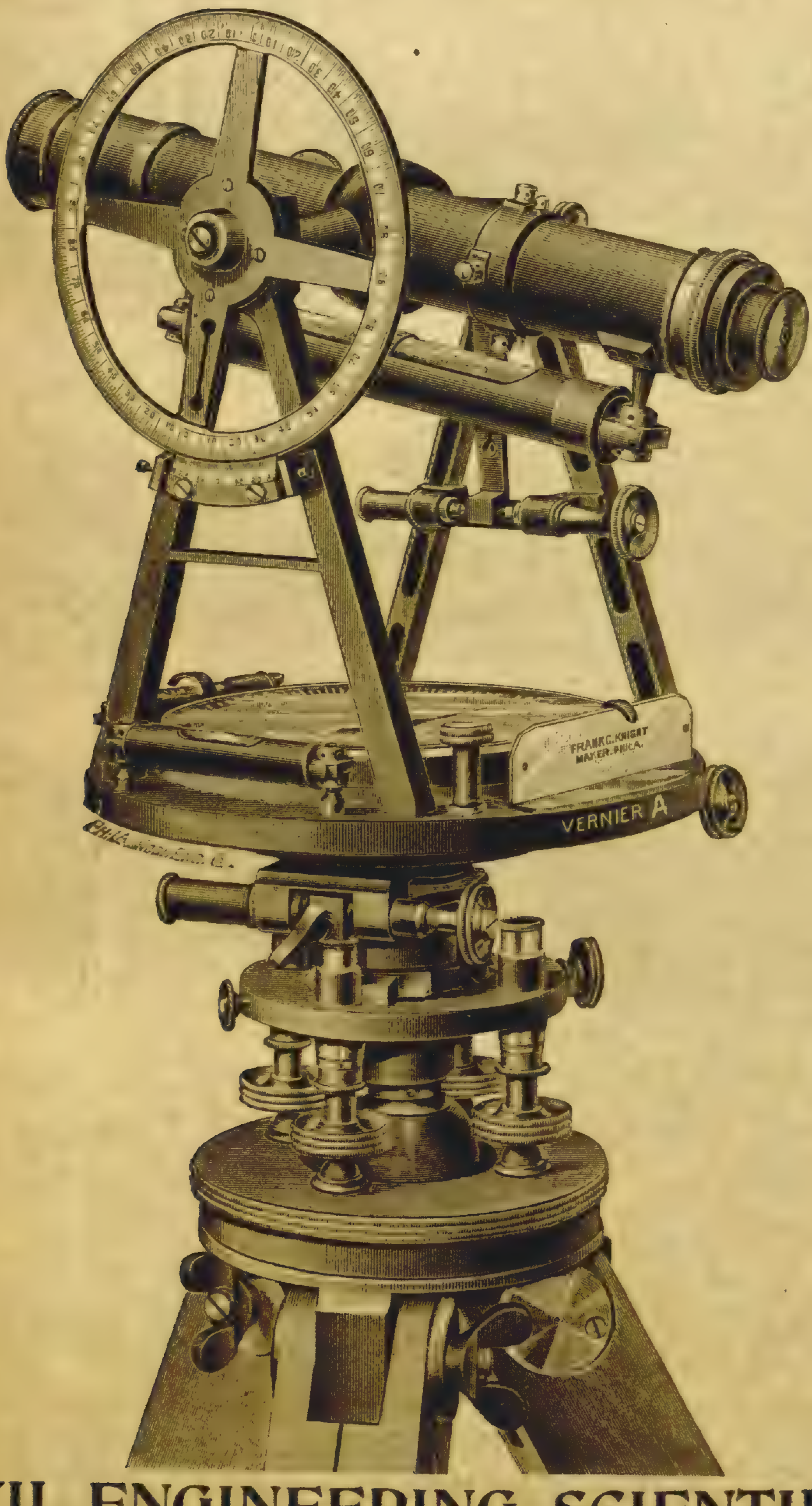

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THE

AMERICAN NATURALIST

VOL. XXX. December, 1896. 360

THE BIOLOGIC ORIGIN OF MENTAL VARIETY, OR HOW WE CAME TO HAVE MINDS.[1]

BY HERBERT NICHOLS.

It is not an infrequent combination that the most familiar things neither excite curiosity nor are understood. Our subtitle suggests an instance of this kind. The naive man commonly takes for granted that he sees the landscape, and hears the orchestra, for no further reason than that they are there before him to be seen and heard. A man a degree wiser gets so far as to recognize that eyes, ears and a brain are necessary. If a biologist be asked, to-day, how we came by this apparatus, he will answer, "through evolution." This is the maximum reach of Science at present. Yet it is nearly as naive to conceive that we have minds, such as ours, merely because we have eyes, ears and a brain, as for one to imagine that he sees just because he has his eyes open. This becomes apparent if we consider the widely accepted doctrine that all the sensory currents running through the nerves to the brain are of the same general sort, as much so as those in electric wires

[1] This paper, under the title "Psychology and Biology" and now somewhat altered from the original, was one of six lectures on "Modern Psychology and its Bearings," delivered, by the author, at Johns Hopkins University in March, 1896.

some of which ring bells while others blow whistles. For if it be asked why our sensory currents 'ring up' such different results as sight from the optic nerve, and hearing from the auditory nerve, it is plainly not satisfactory to answer, "because we have eyes and ears," if, as this doctrine asserts, the eye and ear nerve currents are alike. Nor is it much more enlightening to be told that "it is the *place* in the cortex to which the different nerves run that makes the difference in the sensation resulting from them;" not unless we are in some way told wherein and why these "places" differ. It is just in the fact of never having even inquired how these "places" came to differ, that our evolutionary science falls short in one of the most curiously interesting and important questions that can arise either in biology or in psychology.

Of course, it is a fundamental assumption of both these sciences that all our mental differences are paralleled by molecular differences among the neural activities that underlie them. But this still avoids the question why these last are different, and how they came to be so. And until some answer shall be found that shall logically connect these ultimate neural peculiarities with those peculiarities in outer objects which the world commonly conceives to correspond to our various sights and sounds, it can scarcely be boasted that we are much less naive than the ancients who thought that the objects gave off films that floated into our minds bodily. I by no means imply that this doctrine of all sensory nerve currents being of the same sort is universally accepted. But where any other hypothesis has been offered in its place, the relationship between inner sense and outer stimulus has been left as barren of explanation as even in this doctrine, where, apparently, the possibility of explanation is cut off altogether. But all these matters we are to examine categorically further on. Sufficient has now been said, by way of introduction, to make clear that it is the *variety* of our sensory responses (without which our minds would not be minds), and of their connection with the sorts of stimuli with which they are now connected, that we are, in this paper, to subject to careful investigation. It should be obvious that this inquiry must in-

volve, fundamentally, the evolutionary relation between biology and psychology; and it is for this reason that I have selected it as worthy of the present occasion.

Plunging at once to the heart of our problem, I may state that there are two possible propositions regarding the fundamental relation of our senses to their respective sense organs; which propositions are mutually contradictory and exclusive of each other; which, being fundamental and contradictory, it is necessary to decide between, as a first step toward any permanent insight into the evolutionary relation between body and mind; yet regarding which neither science nor philosophy, up to the present moment, has given any least intimation. It will be the main purpose of this paper to set forth these alternative postulates as completely as I may, within the limit of an hour; and if within that space we do not arrive at any vantage ground, where we may venture a guess at the proper decision between them, I trust that this will but the more emphasize their vast and crucial significance. To venture a prophecy, I may state that the indispensible solution of these two postulates is not likely to be reached for many years to come, nor until wider discussions and further reaching investigations shall have been ploughed under them, than now cover the fields of the great Weissman-Lamarck controversy.

The first of these postulates may be stated as follows: In the light of the little knowledge we as yet possess, it is open to conceive that, in the beginning of the present epoch of animal evolution, crude or primary protoplasm was sensitive not only to all the forms of physical stimulation which now produce sensory responses in us (i. e., sight, sound, taste, smell, touch, temperature, muscle, and other sensations), but was also capable, in response to appropriate stimuli, of an infinite, or x number of other forms of sensation which we know nothing about. In accord, and in illustration of this possibility, we may conceive that the simplest amorphous creatures now actually experience an infinite variety of transient and elementary sensations, including the few we have and a multitude of others that we never have.

Under this conception, we may look upon the rise and development of sense organs generally to mean the slow differentiation of protoplasm to the exclusive use of certain specific forms of stimulation. Thus we may interpret the appearance of eyes to mean the production of an apparatus peculiarly adapted for light waves to the exclusion of all other forms of stimuli. Through the appropriation of the entire fixed surface of our bodies to the particular sense organs which developed in our ancestry, we see how, under this proposition, our few kinds of sense should have been *preserved* to us; and how the infinite number of others with which it endows primitive protoplasm, *should be lost to us through the required forms of stimulation being shut out.*

The alternative of this fundamental formula is that we may conceive, quite oppositely, that protoplasm was capable at first of only one form of sensory response; and of but one mode of neuro-sensory activity correspondent therewith. What this form of sense was we need not consider, at present, further than to suspect that it may have been far different from anything we experience.

Under this proposition we should attribute the rise of various new senses to the development of new kinds of protoplasm, capable of correspondently new forms of sensation. Thus the advent of sight and of sight organs, here, would mean the development of a new basis of physical activities, peculiarly susceptible to light stimulation, and the psychic counterpart of which would be a new kind of sense.

These, then, are our opposing hypotheses. According to one, *Life began with many fleeting, transitory senses, and we have become shut in to a few permanent and highly developed ones.* According to the other, *Life began with one simple sense, and has opened outward with the development of our various and complicated senses.*

It will now be proper to bring forward the implications of these great rival theories in a way to justify the lofty prospectus which we have announced for them.

First we should note, as already has been intimated, that both propositions, alike fundamentally, assume different phases

of neural activity for every psychic happening or sensation, and for every sense quality and shade of quality. That is, one sort for red, another for blue, and others for every sort of taste, smell, and so on.

Next we may note that both propositions equally involve the fact that physical activities immediately underlying our psychic states are enormously complex. Demonstrably, by modern experimentation, they rest upon a chemical basis, intricate beyond all comparison; each molecule comprising various atomic components which, in number, according to highest authority, mount among the billions, and out-run all adequate comprehension.[2]

Next we may observe that in proportion as these molecular activities are complex, so will the molecular differences be great which correspondently lie *between* our different elementary sensations, that is, which correspond to the psychic differences between sights, sounds, smells, tastes and our other major classifications of sensory elements; and between the reds, blues and greens, sweets, sours and bitters, and other minor differences observable in each subclassification, down to the limits of their infinity.

Of course, the old doctrine of specific energies, handed down to us by Johannes Müller, taught us to expect that every different quality of sense must be paralleled by a different form of neural activity. But by emphasizing the enormous complexity of these neural forms, and the vast molecular differences to be observed among our several senses, I wish to bring forward what appears to me to be one of the most important truths in mental science, and one which, in so far as I know, has never before been caught sight of, or taken into account in deciphering the great problem of mind and body.

For finally we may observe that, *Intrinsically and within themselves, these molecular differences, correspondent to the differences among our psychic elements, must necessarily have constituted determining factors of animal evolution; and must have decided what peculiar psychic elements should be selected, and perman-*

[2] See "Man's Glassy Essence," by Charles Pearce, in *The Monist*, October, 1891.

ently incorporated into our psychic existence. They must, therefore, explain the variety of our psychic elements, and the origin of their connection with the central mechanisms on which they now depend, and with their respective sense organs. Consequently, also, they must furnish a key for deciding between our two rival theories of the origin of our psycho-physical organism. And altogether, a study of this subject must unlock to us wide and unforseen fields of scientific truths.

That the evolutionary values of his various "energies" should not have been perceived by Müller is not surprising, but that they remain unconsidered to-day can only be explained by the vagueness of current notions regarding them; and it must now be our task to come to a realization of the conditions which they involve.

Bearing in mind that, as is now demonstrable, the molecular basis underlying each one of the sense-elements of which our brain is capable comprises billions of variable physical constituents, we may first note that this truth must have its influence within the sphere of *Spontaneous Variation.* We do not yet fully know the laws which govern variation, and there is difference of opinion as to the role it plays in developing organisms. But at least the followers of Weissmann should appreciate that not all neural "energies" of such vast complexity could have equal chances of advent, and that this fact must have been a major condition in the origin of those different classes of sensation with which we are now equipped.

Secondly, we may appreciate that the variants in question must constitute determining factors of organization within the circle of *Nutrition.* Not only must our intricate brain components be born into our organism, but they must be maintained there in face of exhaustion and fatigue. Loss of the thyroid gland demonstrates to-day how special are the ingredients necessary for maintaining the general functions of intelligence; and there is ample room within the mysteries of Aphasia to suspect that the requirements are even yet more specific that must be provided within localized regions of the cortex for our different senses. Every notion of modern science suggests that these activities must be specifically com-

plex and vastly variable, and assuming them to be so, the conclusion is inevitable that they should neither be produced or maintained with equal ease; and that, therefore, within the course of adaptation and survival their specific characteristics must have determined, respectively, their own advent and perpetuation.

Third comes the vast region of fitness and selection, which must rise from the relative serviceableness of these several complex activities to *The Environmental Forces*, to whose stimulation they must join themselves for the creature's welfare and preservation. That this sphere is likely to prove of central interest in our problem should be obvious, and because we are to give it much attention further on, we may limit ourselves to the bare statement of it here.

A fourth region of evolutionary choice among possible sense energies must be found in the relative adaptiveness of their molecular complexities to the development and perperfection of such peripheral or end-apparatus processes as are requisite or of profit for mediating between them and the environmental forces. Within this field, more than anywhere else, perhaps, we are likely to discover the functions which most intimately determine the diverse forms of our perceptive organs, and that fix thereby the sorts of mental pictures dependent thereon. This, also, we are to discuss with some fulness presently.

Finally, the factors in question must have determinative bearings within our general physiology. These it will be necessary and important for Science in time to work out; but they are of a nature so remote from psychologic problems proper that they need not be intruded further upon the limited space of this present paper.

Their self-determinative fate within the realms of their Zoologic Genesis, their Physiologic Maintenance and Organization, and their Environmental Adaptation, these, then, are what we have chiefly to study, in our present quest. And these lines being laid down, the following considerations pertinently thrust themselves forward, for our further guidance. It should be obvious at the outset that the nervous currents or impulses

passing from the periphery to the cortex, and arousing there the activities lying nearest to the final sensory results, should be of crucial importance in our investigation of the molecular differences assumed to underly our different senses. Regarding these nerve currents two main opinions are held. Professor Wundt conceives that all the sensory cortical cells are equally potent of all the different sense-forms at our birth; and that the sort of response they actually give is dependent on the form of the impulse that reaches them through the peripheral nerves to which they happen anatomically to be joined.

According to an opposite view, advocated by Prof. James and many others, and to which in our introduction we have already made allusion, the currents in the different sensory nerves are all alike, and the sort of sensory responses they mediate are wholly dependent on the respective places in the cortex to which the different nerves run. The fundamental fact being, that each center is congenitally destined to its one specific form of activity, and the different centers to their permanently different forms.

We can get a sharp notion of these opposing views, as the text-books commonly point out, by imagining the visual and the auditory nerves to be cut somewhere in their course, and the cut ends to be crossed and joined together again so that thereafter the visual impulses will reach the auditory center, and the auditory impulses the visual center. Under this new condition, according to Prof. Wundt, we should both hear and see precisely as before; because both of these cortical centers are capable of both results; because what happens depends on the different forms of the currents that are determined in the peripheral sense organs; and because these run through unchanged, in spite of the crossing and that they are carried to new places. But, according to Prof. James' view, where the currents in the different nerves are all alike, and the results depend wholly on the place in the cortex in which they arrive—under the crossed conditions one should now "see the thunder and hear the lightning."

The chief facts on which the latter notion is founded is, that while the sensory nerves generally are sensitive to several

kinds of artificial stimulation, the sensation resulting thereby is always the same for each nerve. For example, the cut stump of the optic nerve will respond to pinching, pricking, burning, and to chemical and electric stimulation; but always with an indefinite visual flash, whatever the form of stimulus that is applied.

The chief facts upon which those who agree with Prof. Wundt base their opinion, are summed up within certain alleged phenomena of "Substitution," there being some reason to believe that when parts of the cortex are destroyed, either by disease or by experimentation upon animals, certain remaining parts take upon themselves the former functions of the lost parts, and change their former habits and modes of response in so doing.

Neither of these opposing theories are conclusively substantiated at present, since there are counter replies for each. Thus it is open for Prof. Wundt to explain the fact of the optic nerve replying invariably with sight sensations to every sort of artificial stimulation, by saying that this is only true in the adult, where the cells, by having only one form of stimulation brought to them by the nerve to which they are permanently fixed, have been educated persistently in one form of response past the age when they have lost the power of plasticity and of shaking off the old habit to take on a new one—a power which at birth they eminently possessed. And, on the other side, it is open for those who believe in fixed congenital responses to suspect that all the facts of Substitution are due to the lost function being taken up by remaining cells of the same kind, and especially by the correspondent cells of the other half of the body; *i. e.*, those of like kind in the opposite lobe of the brain.

Such is the state of this controversy up to date; and its confusion would be out of place in our present study if we were not now able both to bring this problem, by new considerations, to a solution, and also to demonstrate its cogent bearings upon our main subject. We speedily come to this by recalling that we have already determined that the molecular differences, correspouding to the differences between our several senses, are

certain to have been determining factors of the evolutionary relationship between our minds and bodies in any case, and by then passing on to observe that the sphere of this relationship would be vastly different respectively under our two rival theories regarding afferent nerve currents. Indeed, would be so different as to demand consequences, under one of these theories, so incompatible with existing facts that we shall be able to discard that theory altogether, thus reducing our difficulties, and giving us in the remaining theory an invaluable guide for our main investigation.

This difference in evolutionary sphere comes to view the moment we recognize that under the Wundtian notion (of the sensory currents being different all the way through the nerves to the periphery and to the different environmental forces which respectively stimulate them) the forces determining the selection and perpetuation of these currents in the organism would include all the five regions in which we discovered it was possible for our "molecular differences" (specific energies) to work with selective or evolutionary fitness—namely, the regions of Spontaneous Variation, of Nutrition, of Environmental Adaptiveness, of End-Organ Adaptiveness, and the vaguer sphere of our general physiology. On the other hand, and under the notion that the afferent currents are all alike, it should be plain that this likeness would cut off our central processes, regardless of their molecular differences, from all relative serviceableness either to the great world of environmental forces or to the intricate and indispensable mediating processes in the end-organs, and would thus reduce the sphere of their evolutionary reciprocity to the three remaining and apparently lesser fields.

The significance of this is so great that we shall do well to set forth the connection between our senses and their stimuli, under this point of view, by an illustration. We may do this by imagining two wires coming to this desk, one of which is attached to a bell that is rung in accord with the velocity of the wind outside by an electric current, brought through a wire from a proper apparatus on the roof—a heavy wind ringing the bell violently, and a calm giving no ring at all—and

the other wire we will imagine to be connected with a visible index, the rise and fall of which is determined by the rise and fall of a barometer and other electric apparatus, also situate on the roof. Under this illustration the ringing of the sonorous bell and the moving of the visible index are the analogues of our sensations, the electric wires correspond with our nerves, the wind-gauge and barometer with our end-organs, and the wind and temperature with their external stimuli. And since, under these conditions, by merely changing the wires here at the desk and connecting the barometer with the bell, and the wind-gauge with the index, the "sensory" results would be completely reversed from what they formerly were, so, therefore, we have here a perfect example of what Prof. James means, by saying that all depends on the *place* in the cortex with which the currents or wires are connected. And, going now a step further, these conditions also illustrate what Prof. James has wholly neglected to consider, namely, the evolutionary influences which *made* the "places" in our cortex different, and those which first connected them with the particular end-organs and stimuli with which they are now permanently connected; and these, we quickly perceive, are the important points in our great main problem. Precisely what we want to know is how we came to have the variety of senses that we do have, and how they came to be joined to the particular stimuli to which they are joined. From time out of mind mankind has naively taken for granted that the now existing relationship between sensations and their stimuli is an eternally permanent one of immediate cause and effect. But, as we have pointed out, this cannot be the case if the currents in all the sensory nerves are alike, and if, as Prof. James contends, it is alone the "place" in the cortex which determines the sort of sensation that shall respond to any sort of current or stimulus which may run to it. In this case it should be plain that it is in the characteristic differences of these "places" and in the evolutionary origin of the same, and of their permanent connection with their present peripheral organs, that the secrets must lie which we are in search of. Surely no one, under the conditions of our illustration, would

investigate alone the wind and temperature apparatus on the roof in order to discover why we hear the bell ring instead of see the index move. But, rather he would extend his investigations to discovering how the "nerve" connections originated that now exist, and how the internal apparatus, to which they run, came to be so different that in one case we "see" and in the other "hear" from the same sort of incoming current.

Enabled by this illustration to look with greater clearness into Prof. James' hypothesis, and into some of its implications, we may now go back to the assertions that vastly different spheres of evolutionary influence would be involved as between this theory that these currents are alike and the rival theory that they are different, and to the assertion that certain consequences are logically demanded by the "alike" theory which are so contrary to existing facts that it must be discarded.

What has been neglected by Prof. James is, as I have said, the *evolutionary or selective value* of the sensory currents. If these currents were all alike, then, manifestly the molecular differences which we are obliged to assume in the cortex as underlying our different sensations would be cut off from all diversity of influence either from the end-organ processes or from the environmental forces. *And this is the same as saying that they would be cut off from all selective relationship with these great spheres of influence, and that our end-organs and environment had nothing whatever to do with the origin of our different senses.*

Now, it must not be too quickly inferred from these words in italics, that it would be impossible to account for the evolutionary selection of our several senses within the narrowed sphere of influences remaining after cutting off the peripheral and outer forces. Such an inference would not only be wrong but also would confuse and obscure certain considerations that we are to come to further on, and in view of which it is imperative for me to stop long enough here to point out that the sphere of Spontaneous Variation alone *might* be sufficient to account for the variety of our senses and their present external connections, if *only their origin and not their preservation* needed to be accounted for. And this is done in pointing out that these connections might be originally due wholly to the

period at which a spontaneous variation made a new kind of "sense energy" possible. Thus, the present connection of cerebral sight with the optic nerve and with the eyes, and with the light that falls on them, might well, for all we know to the contrary, be entirely due to the chance appearance of a new form of "energy" or molecular possibility in the cortex just at a time when the development of optic end-organs made a connection with the new cerebral development available, and the exigences of the outer environment made the new linkage of processes of service. Such a connection of inner sense to outer stimulus would be as accidental as anything can be, yet it might be adequate for explaining the facts of our problem *were no influences to be considered that might disturb the permanency of such connections.* And this brings us finally to the influences which most surely would have disturbed the permanency which actually has been maintained, had this theory that the sensory nerve currents are alike been really in force.

These disturbing influences become apparent when we consider the uniformity of the functions that would be left to central nervous processes under the conditions of this theory. No one has ever contended that the outgoing currents of the motor nerves are of diverse kinds. If, therefore, the incoming currents were also all alike, there would then be left to the central processes the entirely homogeneous switch-board function of connecting like currents with like currents. And, under such conditions, and cut off from all diversity of external influences, it seems scarcely possible that some one form of molecular activity or "sense energy" out of the many that variation may have given birth to or protoplasm been originally capable of, would not prove most suitable to this one purpose, and as a consequence become perpetuated to the exclusion of all other less suitable kinds of neural sense-forms. Or, put again more simply, *since moleculur forms are sure to have been evolutionary determinants, therefore, if all the nerve currents were alike it seems certain that the cortical processes must also have become alike.* And since this manifestly is not the case, therefore we must abandon Prof. James' theory.

(*To be Continued.*)

PINEY BRANCH (D. C.) QUARRY WORKSHOP AND ITS IMPLEMENTS.[1]

By Thomas Wilson.[2]

(Continued from page 885.)

II.

Mr. Holmes' paper comprises 26 printed pages. The first part is occupied with a description and statement of facts; the second part is as I have shown made up of theory, assumption, opinion. I have examined them sufficiently to show their want of value. But the climax is reserved to the closing portion, for, commencing on page 19 and continuing 8 pages is a chapter relating to the age of the workshop and the race of the men who worked it. Mr. Holmes' conclusion is that though the quarry is prehistoric the age is not great and the race was the Modern Indian. This he argues with profundity, going into the racial question in detail and with great elaboration.

I decline to argue these propositions. I am appalled at the temerity as well as the dogmatism with which he decides these abstruse questions. He is a gentleman for whom I have the highest regard. I have known him well and favorably for many years. He has studied and written upon art products and art evolution and their relation with prehistoric man, in a philosophical and artistic strain which has done credit to his logic, and been as much benefit to art as to archæology. But Sir John Lubbock, Sir John Evans, Prof. Tylor, Sophus Müller, Hildebrand, Montellius, Naidallac, Hamy, de Mortillet and Cartailhac and the host of eminent Europeans, archæologists and anthropologists, of whom Keane is the latest author, who have spent their lives in the study of this science,

[1] Read before the Anthropological Society, Tuesday Evening, December 4, 1894.

[2] Curator of Prehistoric Anthropology, U. S. National Museum, Washington, D. C.

have not ventured upon the determination of these questions of prehistoric ages and races. with the confidence of Mr. Holmes, and certainly they do not decide these important questions with even a fraction of the satisfaction and certainty which seems to have inspired him.

Mr. Holmes did not content himself with the things of to-day which he saw in the quarry, but turned his mind's eye back when the quarry was being made and depicts it in the time of antiquity, with apparently as much certainty as if he had been then and there present. He not only describes the work with the detail and positiveness I have shown, telling the periods to which it belonged and the race and culture of the men who did the work, but he assumes to decide upon the objects *not* there. He determines not only upon what was left in the quarry, but he decides with equal positiveness upon the ultimate purpose and intention of the workman and the future use and destination of the implements which had been transported elsewhere.

He describes in several places the leaf-shaped blade—the "third stage" of his process—straight and symmetrical, with edges as slightly beveled as consistent with strength, less than half an inch in thickness and shown in *i* to *p*, Pl. IV (my Pl. XIX), and says "when they were realized, the work of this shop was ended" (XX), "they, and they only, were carried away to destinies we may yet reveal" (p. 13). "No examples of the successful quarry products were left upon the ground" (p. 15). "All forms available for further shaping or immediate use were carried away as being the entire product of the shop * * * for final finishing" (p. 15). "This was a stage of advancement which made them portable and placed them fully within reach of processes to be employed in finishing, and that they had been carried away to the villages and buried in damp earth (*cached*), that they might not become hard and (or) brittle before the time came for flaking them into the forms required in the arts. The history of the quarry forms is not completed, however, until we have noted their *final distribution among the individuals of the various tribes*, until we have witnessed the *final step* in the *shaping process*—the flaking out of specific

forms with a tool of bone—and their *final adaptation to use* and *dispersal over the country*," (p. 18).

"Having reached a definite conclusion that the blades were the *exclusively worked product* of the quarry," he "was led to investigate their subsequent history" (p. 18). The italics are mine. His investigation into the subsequent history of these objects led him to define a *cache*. "A 'cache' is a cluster or hoard of stone implements, numbering, perhaps, a score or more, secreted or deposited in the earth and never exhumed. Such hoards are frequently discovered by workmen in the fields," (p. 18).

Pursuing the "subsequent history" of these implements, I propose to go into the region round about Piney Branch, examine the aboriginal village sites of the District of Columbia, the fields containing these alleged secret hoards or caches, and the known places of aboriginal occupation within the neighborhood where these implements were said to have been carried, and see what have actually been found there, what of caches, what of leaf-shaped blades, and what of implements which had been subjected to the (fourth or other) "processes to be employed in finishing, when they were flaked into the final forms required in the arts" (p. 18), and I propose we compare the the objects actually found in these distant places, with what Mr. Holmes said would be found.

I look through my Department in the National Museum for the leaf-shaped implements which, according to the theory of Mr. Holmes, were made at Piney Branch and carried out to the homes of the Indians, their makers, in the District of Columbia, and I find the numbers insignificant; while, as to caches, the Bureau of Ethnology, through Prof. Cyrus Thomas, has lately made a catalogue of the "Known Prehistoric Works in the Eastern United States," among them deposits, hoards, or caches, and there is not a single cache reported from the District of Columbia, this, despite the statement of Mr. Holmes that "such hoards are frequently discovered by workmen in the field."

In the settlement of these questions, it is of high importance that so far as possible, facts and not guesses should be given.

I have taken the trouble to segregate the specimens in my Department in regard to material and locality and to ask a similar report from such private collectors as I could reach. The results I have given in the form of tables, and I have attempted in these to draw a sharp line between the implements which might, according to the Mr. Holmes' theory, have come from Piney Branch quarry, and those which did not.

No implement of *quartz*, found here or elsewhere came from the Piney Branch quarry, nor any of felsite or rhyolite, nor of argillite, shale or ferruginous sandstone, nor of flint, chert, or jasper; for Piney Branch was a quarry of quartzite only.

The following tables show the Aboriginal chipped stone implements from the District of Columbia and its neighborhood, divided according to material, form, locality, and mode of deposit, so as to show the number of quartzite leaf-shaped blades which might have come from Piney Branch quarry, according to Mr. Holmes' theory, and to compare them with those differing in these conditions, and thereby show what number did not come from Piney Branch.

TABLE I. CACHES, HOARDS OR DEPOSITS OF LEAF-SHAPED BLADES. ARRANGED ACCORDING TO LOCALITY AND MATERIAL.

Locality.	Quartz.		Quartzite.		Porphyritic felsite, Rhyolite.		Argillite shale, Fer. Sandstone.		Flint. Jasper. Chert.		[Totals.]	
	Caches.	No. of Implts.	Caches.	No. of Implts.	Caches.	No. of Implts.	Caches.	No. of Implts.	Caches.	No. of Implts.	Caches.	No. of Implts.
Bennings			1	7								
Red Bank			1	5							2	12
Jones Landing												
Piney Branch												
Little Falls					1	a8					1	8
Anacostia												
Pierce's Mills					1	a32					1	32
Piscataway, Md			1	b500							1	500
					1	25					1	25
					1	25					1	25
					1	26					1	26
South River, Ann Arundel Co., Maryland							1	114			1	114
South River, Ann Arundel Co., Md									1	7	1	7
South River, Ann Arundel Co., Md									1	4	1	4
Glenely												
Howard County					1	b100					1	100
Clarksville, Howard County.					1	52					1	52
			3	512	7	268	1	114	2	11	13	905

a, not leaf-shaped; b, estimated.

There have been found in the District (Table I) but two caches of quartzite, containing together only 12 leaf-shaped blades. These are according to Mr. Holmes' theory, "the entire product of the shops" (p. 15), which "had been carried away to the villages and buried in the damp earth (cached) that they might not become too hard and (or) brittle." This was a sorry product of so extensive a quarry as Piney Branch with the "500,000 pieces of waste and failures" found therein by Mr. Holmes; and must have been a sore disappointment to even the cynical and thriftless Indian.

Plate XXIII represents 20 specimens of a cache of 32 arrow or spear heads and leaf-shaped implements found near Pierce's Mill, Rock Creek. Most of the specimens are broken. They are of porphyritic felsite and, therefore, never had any relation with the quartzite quarry at Piney Branch.

Should it be urged that some of the leaf-shaped blades may not have been cached or, if so, that the caches had been disturbed and the blades scattered over the surface, I have made a schedule of these, (table II), which shows a total of 1,948 leaf-shaped blades found on the surface, not cached, of which 1,065 were of quartz, felsite, argillite, etc., and but 883 of quartzite. It

TABLE II. LEAFED-SHAPED BLADES—*NOT* CACHED.

Locality.	Quartz.	Quartzite.	Porphyritic felsite, Rhyolite.	Argillite, Shale, Fer. Sandstone.	Flint. Jasper. Chert.
	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts,
Bennings	4	300	100	100	
	2	30	2	2	1
Red Bank		100	100	40	
Jones Landing		50	20	10	
Piney Branch		15			
		17			
Little Falls	1	215	303	202	
Anacostia	2				
Pierce's Mill					
Cabin John		22	6		1
Piscataway					
U. S. Natl. Mus. Miscl. Collections, from D. C., generally.	148	134	5	10	1
	157	883	541	364	3

Total implements, quartzite,.....883
Total implements not quartzite,.....1065

1,948

is a part of Mr. Holmes' theory that "the working of such a quarry led *inevitably* to the production of blades *in numbers* (meaning in great numbers), and it follows that they were removed "in numbers" (p. 18), but my examination demonstrates the error of this theory, for it shows the blades of quartzite (which alone could have been carried from Piney Branch Quarry) to be in the minority.

Again, Mr. Holmes theorizes (p. 18) that a "time came for flaking them (the blades) into the final forms, knife-blades, scrapers, perforators, and arrow and spear points required in the arts." Therefore, I made still another table (III) to show any of these final forms which might possibly have been made *from leaf-shaped blades;* and, again, we find the theory not

TABLE III. ARROW AND SPEAR HEADS WHICH MIGHT HAVE BEEN MADE FROM LEAF-SHAPED BLADES.

Locality.	Quartz.	Quartzite.	Porphyritic felsite, Rhyolite.	Argillite, Clay slate. iron stone.	Jasper. Chert.
	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.
Bennings	15	15	7	2	
	300	100	50	25	
		2			
Red Bank	200	75	300	50	
Jones Landing	100	50	50	25	
Piney Branch					
Anacostia		2			
Little Falls	102	200	304	50	
Falls Church, Va.,		37	1	8	
		4	1		
Piscataway	3				
U. S. Natl. Mus. Mis. Collections, from D. C., generaliy.	149	209	15	73	8 2
	869	664	728	223	10

Total implements, quartzite, ...694
Total implements not quartzite, ...1840

2534

borne out by facts, for of all these leaf-shaped forms, numbering 2,634, only 694 were of quartzite and could have come from Piney Branch quarry. Thus, it appears that of the leaf-shaped blades found in the District and its environments, cached or not cached, the greater number have been of other material than quartzite, and must have come from other localities than Piney Branch quarry. Is not all this cumulative evidence of error somewhere in Mr. Holmes' theory?

There have been caches found adjacent in Maryland, and it may be suggested that these implements from Piney Branch might have been carried beyond the boundaries of the District of Columbia. But, unfortunately for this theory, the implements which have been found *en cache* in Maryland and adjacent to the District of Columbia are of porphyritic felsite, argillite, and other different material from those in the quarry at Piney Branch, and thus totally dissimilar from them. J. D. McGuire, Esq., of Ellicott City, Md., has furnished the best Maryland collection of these implements known (Table I) and he has kindly furnished me a sample series which have been photographed and are shown in Plate XXIV.

They show 8 caches—one of them 100 and one 114 specimens and a total of 365 specimens, not one of which could possibly have come from Piney Branch for one cache is of flint and jasper specimens, and one of argillite (similar to the leaf-shaped blades found by Dr. Abbot at Trenton), and six are porphyritic felsite or rhyolite.

The leaf-shaped implements found *en cache* in Maryland and some parts of Pennsylvania are, I believe, mostly either of argillite or porphyritic felsite. Several of these caches from the respective localities are to be seen in the Museum, and a single glance is sufficient to establish the absence of their relationship with the quartzite from Piney Branch.

We have now sought for the Piney Branch leaf-shaped quartzite blades at the home of the Indian, throughout the Districs of Columbia and the adjacent parts of Maryland where, according to Mr. Holmes, they were "buried in the damp earth;" andwe have sought in vain. Caches of such implements are not found within the District nor in its neighborhood. It may be hardy to declare a negative and to say that because these quartzite implements have not been found that they do not exist; but how much more hardy and, indeed, perilous must it be for Mr. Holmes to risk everything by declaring the existence of these caches when they have never been found.

The story told by the tables is not completed. Table IV tells of the "flaked implements, knife-blades, scrapers, arrow and spear points and perforators" (which Mr. Holmes says

were common to the region), which were not from the Piney Branch quarry because not made of quartzite. This table shows 21497 such specimens. Table III, showed 2,534 specimens which might have been made from leaf-shaped blades,

TABLE IV. OTHER IMPLEMENTS, SUCH AS KNIVES, SCRAPERS, PERFORATORS, ARROW AND SPEAR HEADS, ETC., APPARENTLY *NOT* MADE FROM LEAF-SHAPED BLADES

Locality.	Quartz.	Quartzite.	Porphritic Felsite, Rhyolite.	Argillite, Clay slate, Iron stone.	Flint. Jasper. Chert.
	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.
Bennings	716	260	13	350	
	3000	4000	200	1500	
Red Bank	700	1200	1500	500	
Jones Landing	300	500	200	100	
Anacostia	35				
Piney Branch					
Little Falls	28	12	23	26	
	500	1000	2000	100	
Pierce's Mill			32		
Piscataway	25	75			
Falls Church	8	6		1	
U. S. Natl. Mus. Miscl. Collections. from D. C., generally.	1720	554	3987 19	253	12 29
	7032	7607	3955	2830	41

Total implements *not* made from leaf-shaped blades..... 21,497

Recapitulation.

Table I. Leaf-shaped blades, *cached*.....865
Table II. Leaf-shaped blades, *not cached*,.....1,948
Table III. Leaf-shaped blades, Impls. made from,.....2,534
Table IV. Impls. *not* made from leaf-shaped blades,.....21,497

26,844

but of these, only 694 were of quartzite. The aggregate of these counts shows 24,031 (21,497 + 2, 534) specimens in these collections not made from Piney Branch quartzite leaf-shaped blades, against 694 which might have been.

Plate XXV shows how arrow and spear heads are, or may be, made from leaf-shaped implements. The five specimens at the top of the Plate are such. They were at one time leaf-shaped implements, and by the making of the notch and stem, they have been changed to arrow or spear heads, that is to say, they have been subjected to the second process which has changed them "into the final forms re-

quired by the arts" (p. 18). The four specimens at the bottom are leaf-shaped blades of quartzite found on the surface at Bennings, D. C., and might or might not have been the product of the quarry at Piney Branch. They form part of the 330 in Table II from that locality. Those in the middle are also leaf-shaped, found on the surface in the District or adjoining it in Maryland or Virginia, but are of quartz, argillite, shale, porphyritic felsite, all of them other material than quartzite, and so they could not have been the product of the quarry at Piney Branch. They form part of 541 in Table II and of the 1948 in Table V.

TABLE V. RECAPITULATION ACCORDING TO FORM.

	No. of Implts.	No. of Implts.	Implts. of quartzite, possibly from Piney Branch.
I.			
Caches, leaf-shaped,	865	865	*a*
Caches, not leaf-shaped,	40		12
II.			
Leaf-shaped blades. Surface, not cached,		1,948	883
III.			
Arrow and spear heads, etc., flaked implements which, from their form, might have been made from leaf-shaped blades,		2,534	694
IV.			1,589—Total which (according to Mr. Holmes' theory) might have been made in or come from the quarry at Piney Branch, out of a total 26,812.
Flaked implements, knife blades, scrapers, arrow and spear points, perforators which were judged, from their form and material, were not made from leaf-shaped blades of quartzite,		21,465	
	Total, all kinds,	26,812	

a. This omits the 500 from Piscataway because it is so far distant from Piney Branch and because we have no report of other implements from that locality. This cache was reported by Mr. Reynolds, who had but a single implement, given him by the finder, as a specimen.

In considering these tables and their bearing on the Piney Branch Quarry, we are to keep continually in mind that the sole and only material in that quarry was quartzite. There was no quartz pophyritic felsite, rhyolite, shale, ferruginous sandstone, flint, jasper or chert found in any of its deposits, and all implements made from any of these materials are to be excluded from consideration because impossible to have come from that quarry. Keeping this in view, these tables show

the following state of facts: Among all those implements from the District of Columbia, but two caches of quartzite were found containing together only 12 leaf-shaped blades of 1.948 leaf-shaped blades not cached, only 883 were of quartzite; of 2,534 common implements, such as arrow- and spear-heads etc., which from their form might have been made from leaf-shaped blades, only 694 were of quartzite, making a total of 1.589 quartzite implements which, according to Mr. Holmes' theory, *might* have come from the Piney Branch Quarry, out of a total of 25,815 implements examined.

Out of all the "1,000 turtle-backs" (p. 14) gathered by Mr. Holmes, their "500,000 brothers and sisters' (p. 12) left, and the "millions of worked stone and unshaped fragments" (p. 7), all "refuse" (p. 12), "waste, failures" (p. 14), of which "these quarries on Rock Creek are the main source," all being done to produce these leaf-shaped blades to be carried away and buried (cached) in the damp earth "that they might be preserved to be made into the final forms required by the arts" (p. 18).—Out of all this toil, the result found up to date is but 2 caches with 12 blades. "The mountain was in labor," etc., etc. Out of a total of 26,812 implements reported in the collections mentioned, but 1,589 were of quartzite leaf-shaped blades that *could have come* from the Piney Branch quarry. Yet the leaf-shaped blades were, according to Mr. Holmes, the "entire product of the quarry" (pp. 13 and 15). What a deal of sack for a pennyworth of bread.

Mr. Holmes' theory that the leaf-shaped blade was the sole product of the quarry workshop, to be afterwards "flaked into the final form" of the common implements of the region, be correct, then the problem may be stated according to the arithmetical law of proportion, as follows: If 1,589 leaf-shaped *quartzite* blades, cached and not cached, finished and unfinished, have been produced from Indian toil and exertion in making the "500,000 turtle-backs," and the "million of worked stones which now occupy the site" (p. 7), all of which are wastes and failures; then how much toil and exertion, and how many millions of worked stones, wastes and failures,

would be required to produce the 26,812 specimens reported in the collections mentioned? I have inveighed against the speculation by which we sometimes attempt to determine, what a great amount of labor the Indian would do for the accomplishment of so little, sometimes the reverse; but we may fairly assume that the aborigine was not such a consummate idiot as to open a quarry as large as this at Piney Branch, and do as much hard work as must have been done there, with the paltry outcome of the insignificant number of quartzite implements shown in the aggregate collections form the District of Columbia. To complete the information on this branch, I have introduced the consolidated tables VI and VII showing the subdivisions according to material and locality.

TABLE VI. RECAPITULATION ACCORDING TO MATERIAL.

Quartz	8,058
Quartzite	9,674
Porphyritic felsite, rhyolite	5,478
Argillite, shale, fer. sandstone	3,541
Chert, flint, jasper	64
	26,815

TABLE VII. RECAPITULATION ACCORDING TO LOCALITY,

Bennings	11,108
Red Bank	4,765
Jones Landing	1,405
Piney Branch[1]	32
Little Falls Church Branch	5,071
Pierce's Mill	32
Anacostia	39
Piscataway, Md	603
Falls Church	66
Mr. McGuire's 8 caches from Md	353
District of Columbia, Museum Collection, locality unidentified	3,341
	26,815

[1] Mr. Holmes' objects are not included.

III.

Mr. Holmes' theory is that the sole implement sought to be obtained by the workman from this quarry, was the thin, leaf-shaped blade, the result of what he calls the third process. His processes Nos. 1 and 2 for making turtle-backs were according to his theory, only designed to lead up to process No. 3, which should produce the thin, leaf-shaped implement.

I think this conclusion does not accord with the facts. Whatever may have been the intention of the workman in making the single or the double turtle-back by processes 1 and 2, (figs. 1, 2, p. 878,) I feel constrained to believe that these were not stages in the production of leaf-shaped implements. I see no evidence of it. I know of no reason why the aboriginal man might not as well have been making the turtle-back for its own sake. It is found all over the United States, it corresponds in a remarkable degree with prehistoric implements from all parts of the world, and no reason is given why it should not have been as much an implement as were the leaf-shaped blades. I do not believe it possible, by any process suggested by Mr. Holmes, nor by the methods apparent from the examination of the leaf-shaped implements themselves, that they were made from the double turtle-back. Mr. Holmes himself is hazy and uncertain about his third process. It consisted, he says, p. 12, "in going over both sides a second and, perhaps, a third time, securing, by the use of small hammers and by deft and careful blows upon the edges, a rude and symmetrical blade." This might mean chipping, or it might mean pecking, hammering or battering. But the process of pecking, hammering or battering is an abrasion by which the substance is worn away grain by grain, passing off in dust; and we know that the leaf-shaped implements were all made by chipping or flaking, and not by pecking, hammering or battering.

I think I may defy Mr. Holmes to make the double turtle-back into a leaf-shaped implement by the process of chipping without treating it as an natural unworked stone and splitting it down through its center regardless of the edge which had been before made, thus destroying its edge and with it the implement. In this operation, the double turtle-back has no advantage over a natural pebble, and it must be treated as such. The operation of striking the turtle-back on the edge to split it and thereby reduce its thickness, has the effect of reducing its size correspondingly. It will have to be reduced considerably when made from the natural pebble, but it will be subjected to a double reduction in size when made from the

turtle-back. The turtle-back and the leaf-shaped implement are practically the same size, except the latter is only $\frac{3}{8}$ or $\frac{1}{2}$ inches in thickness. This reduction in thickness cannot be done without striking the turtle-back on its edge (Plate XXVI) thus working its total destruction and treating it as if it were an original pebble. The plate will make this apparent.

This argument demonstrates that the pretended evolutionary series of Mr. Holmes set forth in his Plate IV, (My Plate XIX) is incorrect. While all the implements are there truly represented, yet they do not form a continuous series. The leaf-shaped implements in the bottom row, "3rd stage, both sides re-worked," could not be made from the "turtle backs" in the two upper rows. Therefore, I deny Mr. Holmes' fundamental proposition. I am fully persuaded that the maker of these implements, whatever else he intended to do, did not intend or attempt to make the leaf-shaped blades out of the turtle-back, or at least that turtle-backs were not a stage in the process of making leaf-shaped implements. If my proposition in this regard be true it breaks Mr. Holmes' theory in the middle.

IV.

Mr. Holmes Says, p. 17, "that to a limited extent, the rude forms—the turtle-back and its near relation—are also found widely scattered over the Potomac Valley outside of the shops on the hills." The suggestion is that these came from this quarry or from similar quarries, and he charges flat-footed that they were the "rejects," "refuse," "debris," "failures."

In January, 1888, the Smithsonian Institution issued a circular, No. 36, asking of its correspondents throughout the United States and Canada, for information as to the number of these implements in their respective localities. This was accompanied with elaborate description and many illustrations, so there should be no mistake in their identity. Answers were then received, from every state in the United States and some from Canada. A consolidation of these answers, with briefs, was published in the Annual Report of the U. S. National Museum for 1888, pp. 766–702, wherein the number reported up to that time is stated at 8,502. This has been largely in-

creased since, and if now subjected to actual count, would be multiplied many times. Many of the specimens, those of quartz and quartzite or other refractory material, were rude like those from Piney Branch, Holmes' Pl. IV (Pl. XIX), but those made of flint or other homogeneous material which chipped easily, were smooth and clean, and on comparison with paleolithic implements from Europe could scarcely be distinguished; those from Texas and Utah especially so.

Bearing on this question, I chose 72 specimens out of some hundreds of the "double turtle-backs," as Mr. Holmes calls them, collected by Mr. Wm. Hunter from the neighborhood of Mt. Vernon, Va., and have had them photographed and made into a Plate XXVII. The specimens on this plate could be duplicated from almost any state. A comparison will show that the same implements are found in every state in the United States. The hammer-stone in the center happens to have been from Piney Branch. The introduction of this is to show that "the double turtle-backs" are found elsewhere than at Piney Branch in considerable numbers; that they are not isolated and sporadic, and that they are shapely and regular, even when made from the refractory quartzite, so much so as to demonstrate them to have been intentional and not accidental forms, and were neither "rejects," "refuse," nor "failures."

V.

Mr. Holmes refuses to consider the implements as furnishing any evidence of their own antiquity. He refuses to compare them with European or other known paleolithic implements, or to accept them as paleolithic because of any similarity of form, appearance, or mode of manufacture. I agree that all existing evidence should be presented and I suppose this has been done in the present case. Accepting this proposition only for the sake of this argument, my reply is that he then has no evidence of antiquity of any portion of the quarry.

Mr. Holmes contends that this great quarry, nigh a quarter of a mile square, had been dug over and excavated, (as is shown by the section, his Plate I), to an average depth of six feet and in many places to eight and nine feet, along the entire hillside

and around its point. He contends that every cubic foot of this section had been dug over, in places to the bed rock, and the stones and clay handled and worked. All the boulders and earth had been loosened and shovelled, and the entire mass re-deposited by the diggers, as the work progressed. Mr. Holmes not only admits this anterior disturbance, but claims it as giving the chief importance to his discovery. His Plate III, a photograph of the quarry face, is introduced by him to demonstrate this prior excavation.

But all this has naught to do in showing the antiquity of the quarry. If he refuses consideration and comparison of the implements and objects found therein, there is nothing to show that all this excavation, trench making and stone breaking may not have been done in comparatively modern times. There is nothing to indicate its antiquity unless it be the appearance of the surface, and this is only by the thickness of soil and the size of the trees; and both of these may have been, the latter must have been, commenced since the early part of this century.

If these trenches, of such length, depth and extent, had been dug by the modern Indian, as declared by Mr. Holmes, we can scarcely imagine that it would have been filled up, raked down, and smoothed over to a regular slope as it now is and was when the trees began to grow on it. Mr. Holmes' Plate I shows the regularity of this slope correctly. Where Mr. Holmes' greatest trenches were dug, the slope from the top of the hill to the bottom is regular and true without any ridge or hollow to indicate an open trench or pit left by the Indian who is alleged to have made it. By whomsoever that quarry was opened and whoever dug those trenches, they were afterward filled up and smoothed over, leaving no break or depression affecting the regularity of the outline of the hill-side. Our knowledge of the modern Indian teaches us that he would not perform this, to him, useless labor. This profound disturbance (the French call it *remaniement*) of the bowlders, clay and earth of the section, leaves no stratification and destroys all evidences of the age of the deposit. There is no fauna.

This, with the item just mentioned, leaves us without evidence as to the antiquity of the quarry work except as furnished by the implements themselves. Their rejection as evidence would leave the question of its antiquity unanswered, and would render the quarry of slight archaeological value. If Mr. Holmes had found stone axes, hatchets or gouges, spear or arrow heads, pieces of pipes, or fragments of pottery, these would have served as evidence of Indian origin, but the utter absence of any of these leaves the Indian theory unsupported; It is a canon of prehistoric archæology, verified by every worker in the field, that no such extensive work as claimed for this quarry could have been done by prehistoric man without having left some of his tools, implements or utensils. But here not an implement or weapon fragment of polished or smoothed stone, not an arrow or spear head, nor pottery, was found. Mr. Holmes says (p. 13), "Only one was found * * * (with) a rude stem worked out at the broad end. This specimen was found near the surface. Two other pieces found at considerable depth exhibit slight indication of specialization of form, which, however, might have been accidental." And this was all.

If it be said that this was a quarry for bowlders with which to make these implements, and that their finding in the disturbed and disarranged deposits is evidence of this fact, I reply, that the surface of the neighborhood is covered with the same kind of bowlders and many of the same kind of implements, and there is no more evidence to show that the implements were made in the quarry than there is that they were made on the surface. For anything shown in the quarry, the whole batch of turtle-backs, double and single, flaked stones, waste, debris, etc., etc., might have been originally on the surface, made there, possibly, in times of antiquity and been tumbled into the ditch, whenever it was filled up.

VI.

Mr. Holmes' paper is radical and final. He not only determines every proposition presented by the implements found at Piney Branch, but he determines them finally, and further

discussion is useless. According to him, we know (from his investigation) all about these implements, all about the man who made them, the race to which he belonged, his use of tools, his machinery and mode of manufacture, his transportation, and a large suggestion concerning his culture. If his conclusion be correct, then Mr. Holmes has determined the entire history of this man as well as that of the implements themselves. His statement is no longer a theory, it is a demonstrated proposition, a proved problem, the work is finished and the book is sealed. It is submitted that this is a greivous mistake.

VII.

I do not attempt any argument to account for this quarry or to explain either the manufacture or use of its implements. It is not my discovery, and I am in no wise bound to sustain or uphold it.

In the discussion, I have said no word about Paleolithic man in America. That question is not involved here. I have elsewhere set forth my opinion on that subject, and I may enlarge upon it on some other occasion, but not here or now.

I have sought only to criticise the theories of Mr. Holmes in reference to the quarry and its implements, and to show what I deem to be the errors in his conclusions, and in doing so I have avoided personalities. I have indulged in no maligning or abusive words, have conceded to him the most honorable intentions, and a truthful rendering of all his facts; and professing for him the kindest and most friendly feeling, I assert that in what I have said, I have given my own fair, and, as far as possible, unbiased opinion and judgment, being moved thereto solely in the interest of truth and science.

FOSSILS AND FOSSILIZATION.

By L. P. Gratacap.

II.

(*Continued from page 912*).

The replacing and mineralizing influence of surface waters may preserve bones which would otherwise quickly disappear. At Big Bone Lick in Kentucky the great numbers of bones of the buffalo are found according to Prof. Shaler "near the present position of the springs and never at any depth beneath the surface." These bones are in some places, "massed to the depth of two feet or more, as close as the stones of a pavement, and so beaten down by the succeeding herds as to make it difficult to lift them from their bed." The attraction of this locality for the herds of wild animals spread through the forests of Kentucky in plistocene and recent times, arose from the saline encrustations made by the natural brines which spring to the surface at this point. There is an ossuary of their remains, the mastodon and elephant bones being upon the higher levels and the buffalo skeletons placed more within the swampy basin, which has itself undergone denudation since the advent of the great proboscideans. These bones are impregnated with salt[1] and have become partially mineralized, while the salt solution itself neutralizes any vegetable acids arising from the decomposition of the reeds which, according to Mr. Cooper, accompany the bones. Yet the falling into swamps or bogs of the great mammals and their gradual submersion and burial in the deeper layers of the tenacious and yielding mixture, has been a means of preserving their remains, especially, as besides their partial immunity from the action of organic acids, their great bones have formed, from their formidable size and texture, an irreducible nucleus. But

[1] The preservation of the bones of the Megalonyx in the Big Bone cave in Tennessee, may be partially ascribed to the presence of large quantities of saltpeter earth.

when we reinvest this continent with herds of wild animals, gregarious in habit, and probably reaching a great numerical aggregate, it seems at first singular that their entire skeletons should be so infrequent. The Mastodon, the Elephant, the Musk Ox,[2] the Caribou Moose[3], and the Reindeer, Horse, Buffalo and Mylodon have been distributed in plistocene and recent times as far south as Kentucky, yet except under peculiar circumstances of sepulture, their remains have disappeared. The conclusion is irresistible that the placement of the bones of vertebrates upon the surface of the ground is unfavorable to fossilization, that they must be covered in by deposits, and while thus held together become hermetically sealed against the accidents of surface conditions and the solution by carbonated and acid waters. The rhinoceros and elephant which were disappearing from Sumatra at the time of Mr. Wallace's visit had, after so recent a withdrawal, left few traces more than crania, tusks and teeth. Prof. Nordenskiold speaking of the polar regions pertinently remarks, "the Polar bear and the reindeer are found there in hundreds, the seal, walrus and white whale in thousands, and birds in millions. These animals must die a 'natural' death in untold numbers. What becomes of their bodies? Of this we have for the present no idea."

The isolated death of individuals from packs of wild animals or the death of those less social in instinct, does not, under most circumstances, insure preservation. When some spot chosen for its proximity to water, or because of its fertility and nourishing vegetation, becomes a rendezvous of groups of animals, the herbivores being followed by the beasts of prey, and the region thus frequented is so situated as to receive the

[2] The Musk Ox, *Ovibos cavifrons* Leidy, was found in Loess of Iowa at Council Bluffs, twelve feet below the surface; also at Ft. Gibson, I. T., St. Louis, New Madrid, Mo., Ohio, Big Bone Lick, Ky. These specimens afforded little else than the head, separated vertebræ and leg bones.

[3] Bones of the fossil elk or moose have been found at Big Bone Lick, Kentucky, but it was reserved for Prof. W. B. Scott, of Princeton College, N. J., to obtain the magnificent example of *Cervalces americanus* Harlan now exhibited in the cabinet of that institution. This almost complete skeleton of a very large extinct elk or moose was discovered in a *shell marl deposit uuder a bog* at Mt. Hermon, N. J. (See Proc. Acad. Nat. Sci. Phila., 1885, p. 181).

PLATE XXIII.

Specimens from a cache found near Pierce's Mill, Rock Creek, D. C. Porphyritic felsite.

PLATE XXIV.

Representatives of eight caches of leaf-shaped implements found in Maryland. None quartzite.

PLATE XXV.

How some leaf-shaped implements are made into "final forms," and some not.

PLATE XXVI.

Leaf-shaped implements cannot be made from "turtle backs" without first destroying them.

PLATE XXVII.

Double "turtle backs" of quartzite found at Mt. Vernon, Va. Samples of thousands, not from Piney Branch Quarry.

drainage of the higher land, a combination of conditions is provided by which a varied fauna may secure an approximately complete representation in fossils. Such localities as those described by Captain Johnston in his Nyassaland are suggested. He says: "At the close of the dry season when the tall grass has been burnt down and there is little or no cover for the game to hide in, it is really a remarkable spectacle as seen from the deck of a steamer, to watch the great herds of big animals wandering over these savannahs in search of the young verdure springing up amid the charred stubble of the old grass.[4] With an opera glass you may distinguish water-buck, gnu, buffalo, eland, pallah, reed-buck and zebra, and occasionally some dark blue-gray blobs, much larger than the other specks and forms which are in their vicinity, turn out to be elephants." Again he says "game in the shape of antelopes and buffaloes was evidently abundant, and no doubt was attracted to the vicinity of this brackish pool by the flakes of salt which remained on the soil when the water had evaporated; and the game in its turn was followed by hyenas, lions and vultures."[5]

In geological time such localities would have afforded a rich commixture of fossil remains if the circumstances favorable for the deposition of a protecting stratum of earth existed. Yet in all instances the social relations of the animals have an importance, and those social relations are somewhat modified by the topography of the country they inhabit. The wide plains of south-eastern or subcentral Africa, with an unchecked communication for miles, numerous rivers and rich vegetation,

[4] Such scenes described by Capt. Johnston are closely imitated in the picture, drawn by W. Boyd Dawkins, of the Bristol Channel in plistocene times, when it was a fertile plain "supporting herds of reindeer, horses and bisons, many elephants and rhinoceroses, and now and then being traversed by a stray hippopotamus, which would afford abundant prey to the lions, bears and hyenas inhabiting all the accessible caves, as well as to their enemy and destroyer, man." See also Dr. E. Holub's "Seven Years in S. Africa," Vol. I, p. 267.

[5] See also East Africa and its Big Game by Sir John C. Willoughby, wherein he describes the open plain with buffalo, zebra, hartebeest, eland, rhinoceros, ostrich, Grant's antelopes teinbock and wart-hog scattered over it, and in another place where he saw "lions, rhinoceros, lesser kudu, wild dogs, hyenas, cheetah, water-buck and zebra, the total seen amounting to nineteen varieties."

permit a most heterogeneous assemblage of animals, whereas a disturbed and mountainous country divided into lofty plateaux, low savannahs and barricaded valleys would separate and isolate related groups, and from a low zoic maxima,[6] in consequence of its irregularity and limited geographical scope, afford a meagre and less diversified fauna, and actually fewer fossils. The successful fossilization of the bones of terrestrial vertebrates can best be secured by their rapid burial within impervious beds of enveloping material wherein they undergo a slow process of partial or complete petrifaction. Nordenskiold in his Voyage of the Vega discovered a very extensive deposit of whale bones in a sand dune upon a beach of Siberia in the Chuckchi country. These apparently had fallen to the bottom of a sea and were entombed by new layers and beds of sand. They were thus subfossil and possessed an immense age. Where they had become exposed by the violence of the waves they were decaying, but where buried in the sand the well preserved bones of these cetaceans were found in innumerable quantities.

The dispersal of bones in the ocean seems as unfavorable to their preservation as exposure on the surface of the earth or in the vegetable acids of its superficial covering. It is notorious that the dredging expeditions deputed to explore the depths of the oceans have seldom encountered the remains of vertebrate animals. The skeletons of whales, seals, porpoises and sharks have not been found commonly, though teeth brought up from these depths indicate the dissolution of the animals in the oceanic waters. So striking is this absence of osseous remains that Lyell remarks "there are regions at present, in the Indian and Pacific Oceans, coextensive in area with Europe and North America where we might dredge the bottom and draw up thousands of shells and corals without obtaining one bone of a land quadruped."[7]

[6] See AMER. NAT., Vol. XX, p. 1009.

[7] "Some astonishment was created on board the 'Challenger' that the dredge after having dragged over miles and miles of the bottom of the sea, and up and down almost every oceanic basin, should never bring up any bones of fish or whale, or any remains of other large animals which inhabit the sea or whose bodies may have been carried down to the sea." Thalassa, J. J. Wild, p. 133.

The explanation of this fact has hardly advanced further than the theoretical stage. It may be attributed to the dissolving agency of carbonic anhydride in the lower strata of water, to the insidious action of the products of organic change, which Julien seems inclined to exaggerate, or to the more ordinary factors of animal consumption. Prof. Verrill and Mr. Sanderson Smith hold that the disappearance of bone in the ocean is due to its being attacked and eaten by crustacea.[8] If this is true and solution has no practical bearing as an agent in their disappearance, it seems likely that some maceration and softening produced by pressure and soakage reduce the bone to such a consistency as to render it more easily attacked by these animals, and incidentally render the bones themselves liable to separation and absorption by the sea waters. It is peculiar that while great numbers of shells are raised from the bottom of the ocean, the same areas seldom or never produce mammalian relics, the otoliths or ear bones of whales, and the teeth of sharks excepted. The skeletons of sharks and fishes might naturally undergo softening or become from their semicartilaginous nature the prey of smaller animals, but the hard parts of whales seem well calculated to resist attack. These larger animals upon their death may be dragged by submarine currents into the deeper parts of the ocean and there become subjected to a stronger chemical action than is observable at less depths. The theory of Mr. Murray as to the formation of coral reefs would make it appear that bottom waters contain more carbonic acid than those on the surface, but this Prof. Dittmar[9] calls in question, thinking that solution is effected by the prolonged contact with the sea water itself. At any rate "the alkalinity of bottom waters was

[8] It is a matter of some interest to learn that "remains of the Atlantic Walrus in a fossil state, have been found at various points along the Atlantic Coast from Maine to South Carolina, and in Europe as far south as England and France" (J. A. Allen). The most striking instance is that of a skull found on the sea beach at Long Branch, New Jersey, of which Dr. Leidy said it "has lost a porsion of the cranium proper, and the exserted portion of one tusk, but otherwise, except being a little water worn, is in a good state of preservation."

[9] The Physics and Chemistry of the voyage of H. M. S. Challenger, Vol. I, pt. 1, Composition of Ocean Water by Prof. W. Dittmar.

found to be distinctly greater than that of those from the surface, and this increase was exactly proportional to the larger quantity of lime present in the former," and upon this fact we might found a belief that bones disappeared through solution. At considerable depths pressure would greatly reinforce chemical action, and as carbonic anhydride is liquified under a pressure of something over 38 atmospheres, in the deeper basins of the ocean this reagent may exist as a liquid. In a French experiment, water, taken from a great depth of ocean, was under so high a tension from the enclosed gas, that upon release it spurted in a jet from the containing vessel.

The remains of terrestrial vertebrates represent most generally the submergence and death of the living animals themselves, and Lyell has well described the way. He says (Principles, (Vol. II, p. 542), "river inundations recur in most climates at very irregular intervals and expend their fury on those rich alluvial plains, where herds of herbivorous quadrupeds congregate together. These animals are often surprised, and, being unable to stem the current, are hurried along until they are drowned, when they sink at first immediately to the bottom. Here their bodies are drifted along, together with sediment, into lakes or seas, and may then be covered by a mass of mud, sand and pebbles thrown down upon them.

"Where the body is so buried in drift sand, or mud accumulated upon it, as never to rise again, the skeleton may be preserved entire; but if it comes again to the surface while in the process of putrefaction, the bones commonly fall piecemeal from the floating carcass, and may in that case be scattered at random over the bottom of the lake, estuary, or sea, so that a jaw may afterwards be found in one place, a rib in another, a humerus in a third—all included, perhaps, in a matrix of fine materials where there may be evidence of very slight transporting power in the current, or even of none, but simply of some chemical precipitate."

Entire skeletons of animals or their bones scattered over declivities or plains are not so likely to be gathered together and deposited in some spot, because, as we have seen, they become subject to decay and dissipation., and because it would

appear that any wide-spread catastrophes killing large numbers of a herd are rare, and in the case of individual deaths, the remains, if carried by freshets to some lower level, would seldom undergo the same vicissitudes and be buried at the same point. Yet the wholesale destruction of mammals in a state of nature may be considered a possibility, though improbable. Sir Samuel H. Baker speaks[10] of cattle introduced at his camp at Fatiko, in Africa, who could not live there, "as the herbage was quite different to that to which they had been accustomed." They died so rapidly and in such numbers that in three months only three or four remained out of as many thousand. In the over stocked ranches of California, thousands of heads of sheep have been seen lying dead in vast heaps in ravines and valleys, as if nourishment had become exhausted by draught or by actual deplenishment of the available pasturage. Is not a similar mortality possible under natural conditions when, as in the case of the Fatiko cattle, animals have been driven by storms into localities incapable of their support, or when, as with the ranch sheep of California, an area previously put under severe strain for the support of its feral population, by an accident of weather or season fails entirely to furnish its occupants with food?

Wallace in his Malay Archipelago speaks of the destructive effects of drought upon animal life in the Arne Islands, where from an excessive scarcity of water, "sometimes hundreds of birds and other animals die." The effects of sudden and violent falls of hail and snow are noticed by Stansbury, who found on the shores of the Great Salt Lake, a large number of young pelicans killed by the severity of a hail storm. The possibility of numbers of animals becoming buried at once, may be illustrated in the condition of the banks of the River Vaal in South Africa, of which Dr. Holub says that "its banks almost to the very middle of the channel are so soft and slippery, that draught animals going to drink are liable to sink so deep into the mud that it is impossible to extricate them." The internecine strife of wild animals may itself result in the accidental death of numbers, as when the hunters

[10] Ismailia, Sir S. H. Baker. p. 294.

—the carnivora—pursue their prey and drive them into lakes or rivers or, perhaps, force them over precipices. Dr. Hayden has observed similar occurrences, and in the same place where he records this observation corroborates the interesting suggestion of Lyell as to animals meeting their death by falling through thin ice. He says: "The wolves watch the deer, antelope and other feebler animals as they go down to the streams to drink, and all over the wide bottoms are the skeletons of these animals in a more or less perfect condition. It is not an uncommon occurrence for a band of wolves to attack an aged buffalo too old to offer a successful resistance. He must always betake himself to the river, where he is not unfrequently drowned, or is destroyed by the wolves on a sand bar or island. Annually thousands of buffaloes are drowned in attempting to cross the Missouri on the ice, as it is breaking up in the spring. Their bodies have been seen floating down the Missouri at Fort Union and Fort Clark by hundreds, and lodging on some of the islands or sand bars in the river. In the spring of 1858 several thousand bodies of buffaloes passed down the Kansas River below the mouth of Solomon's Fork and were carried into the Missouri."

Dr. Holub speaks of the devastation amongst oxen, elands, hartebeests, sheep, goats, wild pigs, etc., wrought by the attacks of hyenas in South Africa as "really frightful," and in pliocene times on our globe similar causes may have led to the collection of important groups of fossils. Even the instincts of animals may lead to their wholesale destruction. Mosely speaks of the migration of turtles at Ascension Islands saying, "the young turtles on leaving the egg go down to the sea and disappear, returning only when full grown to breed; this is the account given by the residents. If they do really leave the neighborhood of the island, there seems no possible means by which they can find their way back."

Although it is impossible that any of the fish beds, found as fossils, can, at least in paleozoic or mesozoic rocks, have been formed in the way instanced by Nordenskiold as a cause of death amongst arctic fish, yet the circumstance is intrinsically interesting, and the the reflections it suggests as to the likeli-

hood of general destruction from other causes of the fish in geological strata, wherein they abound, make it a useful point of reference in this matter. Nordenskiold says (Voyage of the Vega), "a large number of fish (*Gadus polaris*) were seen above the foot of a large block of ground ice, near which we lay to for some hours. Next day we saw near one of the islands, where the water was very clear, the sea-bottom bestrewed with innumerable fish of the same species. They had probably perished from the same cause, which often kills fish in the river Obi in so great numbers that the water is infected, namely, from a large shoal of fish having been inclosed by ice in a small hole, where the water, when its surface has frozen, could no longer by absorption from the air replace the oxygen consumed, and where the fish have thus been literally drowned."

However accumulated, whether by the sudden death of large numbers of animals by floods and storms along the banks of streams or the margins of lakes, or whether from other natural causes animals perish in numbers and upon restricted areas, their bones are carried by water action into the depressions, sinks, crevices and basins of a country, and being there sealed up from decomposition or dispersion by the silt and gathering accretions of various mineral deposits above them, they become fossils. The caves in limestone districts receive a contribution of animal remains partially brought into them by surface water partially by the the predatory instincts of carnivorous mammalia or birds. Prof. Hartt in his Geology of Brazil instances the interesting examples in the Sao Francisco basin wherein the numerous caverns, extending sometimes two thousand feet into the rock, furnish abundant remains both of long extinct mammalia as the Glyptodon, Mastodon, Mylodon, Megatherium, Chlamydotherium, Toxodon and Macrauchenia, and innumerable remnants of the smaller living animals brought there by owls, whose bones mingle with those of other occupants, as bats and felidæ. These variously distributed in the different caves were mingled in a red clay earth more or less cemented and encrusted by a stalagmitic crust. Similarly fossil vertebrates have been entombed in the caves of England, France, Belgium, Spain,

Sicily, Germany, Russia and Australia. Many of these caves open by vertical fissures to the surface, and down these irregular holes and chimneys the bones have been washed, while in some cases as at Wirksworth, in Derbyshire, England, or at the *cave* in the limestone at Port Kennedy, Pa., described by C. M. Wheatley, animals may have plunged into them and died in their imprisonment. Once gathered in their final resting place the process of burial goes on, in many cases rapidly, and in others slowly, and, according to the completeness of their sepulture, the condition of the bone is more or less perfectly preserved. In river floods the animals or osseous debris borne forward in their waters are soon enveloped in the midst of the accompanying clay, sand, gravel and calcareous mud torn from the channel and banks of the stream. They sink to the bottom and are rapidly covered in by a rising blanket of deposition. In caves and hollow receptacles the infiltrating streams bring constant additions of mud, and by their erosive action upon the surrounding limestone which they dissolve, they redeposit carbonate of lime through the interstices of the granular accumulation or form a hard layer above it, upon which again later ossuaries may be made, and the cave floor offer a study of separate and successive periods.

Amongst the multitudinous details connected with the exploration of caves for the traces and evidence of prehistoric man, the following facts have some reference to the fossilization of terrestrial vertebrates. A cave at Gailenreuth, Germany, contains an enormous quantity of bones and teeth of animals formerly living in its vicinity, and according to Dr. Buckland, introduced by a stream which passed through this chain of caverns in its subterranean course to lower levels. In the cave of Kühloch, Germany, a black animal dust covers the whole floor to the depth of six feet, derived says the same authority "from comminuted and pulverized bone." This accumulation is attributed to the use of the cave by bears through centuries.[11] In the caves of North Wales near St.

[11] The habits of wild animals in resorting to caves is fully established by observation. The custom of the panthers to make lairs of natural caves is instanced by Prof. Baird, and Major Pinto speaks of a circular chamber in a limestone mountain in Western Africa as a "regular haunt of wild beasts, as one might

Asaph, the bones are in a similarly pulverulent state and produce clouds as they are disturbed. In a cave at Banwell in the Mendip Hills, England, thousands of bones of bison, horse and reindeer were taken out of a red silt which filled the cave to its roof. The entire deposit has been introduced by water through a vertical fissure which opened on the surface. In the Hyena Den, at Wookey Hole, "the organic remains were in all stages of decay, some crumbling to dust at the touch, while others were perfectly preserved and had lost very little of their gelatine." In an arm or section of this same cavern according to Dawkins, "most of the bones were as soft as wet mortar," an interesting statement which throws light upon the probable state of maceration which bones attain before disappearance by trituration or solution. The mineralization of the bones in the various caves, so patiently explored, presents striking differences. In some the bone seems reduced to the last stages of cohesion, while in others it has become filled with carbonate of lime or partially silicified, and attains a considerable gravity.

(To be continued.)

THE GEOGRAPHICAL DISTRIBUTION OF BATRACHIA AND REPTILIA IN NORTH AMERICA.

By E. D. Cope.

(Continued from page 902.)

III. The Eastern Subregion.

The fauna of Batrachia and Reptilia of this subregion is characterized by what it lacks as much as by what it possesses. The number of species which occupy its entire extent exclu-

judge from the air which was perfectly saturated with the pungent smell of certain animals, as well as from the traces of a lion impressed on the impalpable powder which covered the ground, where we met with a few quills of the *Hystrix africana*."

sively of other subregions is small, while a larger number are restricted to parts of it. Verrill divided it into four districts, viz.: the Carolinian, the Alleghenian, the Canadian, and the Hudsonian. These are distinguished by the ranges of mammals and reptiles, and the breeding-places of birds. The Carolinian fauna extends in a belt north of the Austroriparian subregion, from Long Island, south of the hill region of New Jersey, to the southeastern corner of Pennsylvania, and thence inland. It embraces a wide belt in Maryland and Virginia, and all of central North Carolina, and then narrows very much in passing round south of the Alleghenies of Georgia. It extends north again, occupying East Tennessee, West Virginia, Kentucky, Indiana, the greater parts of Illinois and Ohio, and the southern border of Michigan. It includes southern Wisconsin and Minnesota, all of Iowa, and the greater part of Missouri. The Alleghenian embraces the States north of the line just described, excepting the regions pertaining to the Canadian fauna, which I now describe. This includes northern Maine, New Hampshire and Vermont, with the Green Mountains, the Adirondacks and summits of the Allegheny Montains as far as Georgia. It includes Canada east and north of the lakes. The Hudsonian fauna is entirely north of the isothermal of 50°. It has great extent west of Hudson's Bay, and is narrowed southeastward to Newfoundland.

The information as to the distribution of the Batrachia and Reptilia now at hand, points to the following conclusions. The Hudsonian fauna need not be further referred to here, as it is part of the Holarctic region. The Canadian is sustained, as defined by the range of certain Batrachia. The demarkation between the Alleghenian and Carolinian is determined by the northern limit of most of the species common to the Eastern and Austroriparian subregions. An important division is indicated by the boundaries set to the range of certain species by the Allegheny Mountains. This division affects chiefly the Carolinian district of Verrill, and I therefore propose to abolish that name, and replace it by the two terms Cisalleghenian for Eastern, and Transalleganian for the Western districts. They are separated from each other by the Alleghenian district of

the foot hills, and the Canadian of the summits of the Allegheny Mountains.

The species which are found over the entire eastern subregion, and not elsewhere, are the following:

Amblystoma jeffersonianum Green.
Plethodon cinereus Green.
Rana silvatica Lec.
Rana palustris Lec.
Osceola doliata triangula Boie.
Natrix fasciata sipedon Linn.
Eutænia sirtalis graminea Cope.

The Canadian district is characterized by the following species, which are restricted to it:

Amblystoma jeffersonianum laterale Hallow.
Gyrinophilus porphyriticus Green.
Desmognathus ochrophæa Cope.
Desmognathus nigra Green.
Bufo lentiginosus fowlerii Putn.
Rana cantabrigensis Baird.
Rana septentrionalis Baird.

The list above given as universally distributed in the Eastern subregion characterizes *the Alleghenian district.* I know of no species that is restricted to it. The genera which do not extend north of it are the following:

BATRACHIA:

Chorophilus,
Hyla,
Hemidactylium,
Cryptobranchus.
Necturus.

SAURIA:

Sceloporus,
Eumeces.

SERPENTES:

Carphophiops,
Coluber,
Cyclophis,
Natrix,
Ophibolus,
Heterodon,
Ancistrodon.
Systrurus,
Crotalus,

The two remaining districts include the large number of species which are common to the Eastern and Austroriparian subregions enumerated under the latter head. The Cisalleghenian is further characterized by the following:

Hyla andersonii Bd.
Rana virgatipes Cope.
Ophibolus rhombomaculatus Holbr.

To these must be added from the Austroriparian list:
Abastor erythrogrammus Daud.

The following species are peculiar to the *Transalleghenian district:*

Chondrotus microstomus Cope.
Spelerpes maculicaudus Cope.
Rana areolata circulosa R. & D.
Carphophiops vermis Kenn.
Coluber vulpinus B. & G.
Ophibolus calligaster Say.
Eutænia radix B. & G.
Eutænia butlerii Cope.
Tropidoclonium lineatum Hallow.
Natrix kirtlandii Kenn.
Systrurus catenatus Raf.

Probably *Eutaenia brachystoma* Cope belongs to this district but only one specimen has been found.

The following species enter this district only from the Austroriparian:

Natrix grahamii B. & G.
Eutænia proxima Say.

Of the species peculiar to the Transalleghenian district, *Ophibolus calligaster* and *Tropidoclonium lineatum* extend into the northern limits of the Texan district.

The genera which do not range northward of the Cisalleghenian district are Cnemidophorus, Liolepisma and Abastor.

The total number of species of the Eastern subregion is thus:

Generally distributed,	7
Peculiar to Cisalleghenian,	3
Peculiar to Transalleghenian,	9
Peculiar to Canadian,	7
Common to Austroriparian,	34
	60

IV. The Austroriparian Subregion.

This subregion is the range of a large number of species of Batrachia and Reptilia, only a part of which occupy it to the exclusion of all other subregions, and another series of which occupy parts only of its area. Three centers of distribution

within its borders may be discerned—the Ocmulgian, the Louisianian and the Texan. The Texan is especially characterized by the combination of the Austroriparian fauna with a considerable number of the species of the Sonoran subregion. The characteristic Austroriparian species are the following:

TRACHYSTOMATA:

Siren lacertina L.

URODELA:

Amphiuma means Gard.
Amblystoma talpoïdeum Holbr.
Manculus quadridigitatus Holbr.

SALIENTIA:

Bufo lentiginosus lentiginosus Shaw.
Chorophilus occidentalis B. & G.
Hyla carolinensis Penn.
Engystoma carolinense.

LORICATA:

Alligator mississippiensis Daud.

SAURIA:

Ophisaurus ventralis Daud.
Anolis carolinensis.

SERPENTES:

Heterodon simus Linn.
Cyclophis æstivus Linn.
Zamenis flagelliformis Catesb.
Coluber spiloides D. & B.
Compsosoma corais couperii Holbr.
Osceola doliata syspila Cope.
Osceola doliata coccinea Schl.
Ophibolus getulus sayi Hobr.
Cemophora coccinea Blum.
Natrix clarkii B. & G.
Natrix fasciata fasciata L.
Natrix fasciata erythrogaster Shaw.
Natrix cyclopium D. & B.
Virginia valeriæ B. & G.
Haldea striatula L.
Tantilla coronata B. & G.
Elaps fulvius L.
Ancistrodon piscivorus Lacep.
Systrurus miliarius L.
Crotalus adamanteus adamanteus Beauv.

Thirty-one species and subspecies.

The Austroriparian shares with the Floridan subregion all of the above species except *Coluber spiloides, Natrix clarkii, Virginia valeriæ* and *Haldea striatula*, so far as yet known. It shares with the Eastern subregion the following thirty-four species.

PROTEIDA :

Necturus maculatus Raf.

URODELA :

Cryptobranchus alleghaniensis Daud.
Amblystoma opacum Grav.
Amblystoma punctatum L.
Amblystoma tigrinum Green.
Plethodon glutinosus Green.
Spelerpes guttolineatus Holbr.
Spelerpes ruber Daud.
Desmognathus fusca Raf.
Diemyctylus viridescens Raf.
Bufo americanus americanus Lec.
Scaphiopus holbrookii Harl.
Acris gryllus Lec.
Hyla versicolor Lec.
Rana pipiens pipiens Kalm.
Rana areolata B. & G.
Rana clamata Daud.
Rana catesbiana Shaw.

SAURIA :

Sceloporus undulatus Latr.
Cnemidophorus sexlineatus L.
Eumeces quinquelineatus L.
Liolepisma laterale Say.

SERPENTES :

Abastor erythrogrammus Daud.
Carphophiops amœnus Say.
Heterodon platyrhinus Latr.
Diadophis punctatus L.
Liopeltis vernalis L.
Zamenis constrictor L.
Coluber obsoletus Say.
Pityophis melanoleucus Daud.
Ophibolus getulus getulus L.
Eutænia sirtalis sirtalis L.
Ancistrodon contortrix L.
Crotalus horridus L.

The following species are restricted to the eastern part of the Austroriparian subregion, not extending west of the Atlantic drainage. To this district I have the name of *the Ocmulgian.*

PROTEIDA :

Necturus punctatus Gibbs.

URODELA :

Stereochilus marginatum Hallow.
Chondrotus cingulatus Cope.

SALIENTIA ;

Bufo quercicus Holbr.
Chorophilus ornatus Holbr.
Chorophilus oculatus Holbr.

SERPENTES :

Abastor erythrogrammus Daud.
Rhadinæa flavilatus Cope.
Coluber quadrivittatus Holbr.
Natrix rigida Say.

The following species are restricted to the Ocmulgian and Louisianian districts with present information. First, all the Batrachia which the Austroriparian subregion shares with the Eastern, excepting *Amblystoma tigrinum*, *Diemyctylus viridescens*, *Acris gryllus*, *Rana areolata*. Second, *Farancia abacura* Holbr., *Coluber guttatus* L.

The following species are to be added to the general Austroriparian (p. 1007) to form the list of the Texan district:

PROTEIDA:

Typhlomolge rathbunii Stejn.

URODELA:

Diemyctylus meridionalis Cope.
Chondrotus texanus Matth.

SALIENTIA:

Bufo debilis B. & G.
Bufo punctatus B. & G.
Bufo valliceps Wiegm.
Bufo compactilis Wiegm.
Lithodytes latrans Cope.
Chorophilus triseriatus clarkii B. & G.

SAURIA:

Holbrookia texana Trosch.
Holbrookia maculata B. & G.
Crotaphytus collaris Say.
Sceloporus spinosus Wiegm.
Sceloporus consobrinus B. & G.
Phrynosoma cornutum Harl.
Eublepharis variegatus Bd.
Gerrhonotus liocephalus Wiegm.
Eumeces epipleurotus Cope.
Eumeces pachyurus Cope.
Eumeces brevilineatus Cope.
Eumeces tetragrammus Bd.
Eumeces obsoletus B. & G.

SERPENTES:

Diadophis amabilis docilis B. & G.
Diadophis amabilis stictogenys Cope.
Hypsiglena ochrorhynchus Cope.
Rhinochilus lecontei B. & G.
Coluber emoryi B. & G.
Osceola doliata annulata Kenn.
Ogmius episcopus episcopus Kenn.
Natrix rhombifera Hallow.
Natrix fasciata transversa Hallow.
Virginia elegans Kenn.
Eutænia proxima Say.
Eutænia elegans marciana B. & G.
Eutænia eques ocellata Cope.
Tantilla gracilis B. & G.
Tantilla nigriceps Kenn.
Systrurus catenatus edwardsii B. & G.
Crotalus adamanteus atrox B. & G.

Sixty-one species and subspecies, making a total for the Austroriparian as follows:

Generally distributed,	31
Shared with the Eastern subregion,	34
Ocmulgian only,	10
Louisianian and Ocmulgian only,	2
Texan exclusively (in the subregion),	38
	115

The species which enter the Texan territory from the Sonoran extend to various distances to the north and east. Thus, *Crotaphytus collaris* ranges to southern Missouri, and *Holbrookia maculata* to Arkansas. *Sceloporus spinosus* extends along the Gulf States to western Florida. *Phrynosoma cornutum* extends eastward to Dallas, Texas. *Rhinochilus lecontei* on the other hand has not been found east of Austin. Several species from the extreme southwest of Texas have not been included in the above lists, since some of them are well-known to belong to the Central American fauna, while the range of others is probably similar, but is not sufficiently known. Of the former kind are *Drymobius margaritiferus* Schl., *Sibon albofuscum* Lac., and *Coniophanes imperialis* B. & G.; of the latter are *Lystoptychus lateralis* Cope, *Holbrookia propinqua* B. & G. and *Hypopachus cuneus* Cope.

V. The Floridan Subregion.

The species and subspecies peculiar to this subregion are the following:

Batrachia:

Pseudobranchus striatus Lec.
Hyla gratiosa Lec.
Rana areolata æsopus Cope.

Sauria:

Eumeces egregius Bd.
Rhineura floridana Bd.

Serpentes:

Coluber rosaceus Cope.
Coluber guttatus sellatus Cope.
Osceola doliata parallela Cope.
Stylosoma extenuatum Brown.
Eutænia sackenii Kenn.
Seminatrix pygæa Cope.
Natrix usta Cope.
Natrix compressicauda Kenn.
Natrix fasciata pictiventer Cope.
Liodytes allenii Garm.

Species which are wanderers from the West Indian region are:

Lithodytes ricordii D. & B.
Sphærodactylus notatus Bd.
Crocodilus americanus Seba.

The *Rhadinæa flavilatus* Cope ranges throughout both the Floridan subregion and the Ocmulgian district. Two other species may be characteristic of the Floridan subregion, but only one specimen of each has been obtained. These are *Manculus remifer* Cope, and *Elaps distans* Kenn.

Species which the Floridan subregion shares with the Austroriparian are the following:

TRACHYSTOMATA:

Siren lacertina L.

AMPHIUMOIDEA:

Amphiuma means Gard.

PSEUDOSAURIA:

?Plethodon glutinosus Green.

SALIENTIA:

Bufo lentiginosus lentiginosus Shaw.
Bufo quercicus Holbr.
Hyla squirella Bosc.
Hyla femoralis Latr.
Hyla carolinensis Penn.
Acris gryllus Lec.
Chorophilus nigritus Lec.
Scaphiopus holbrookii Harl.
Rana pipiens sphenocephala Cope.
Rana castesbiana Shaw.

LORICATA:

Alligator mississipiensis Daud.

SAURIA:

Sceloporus undulatus Latr.
Cnemidophorus sexlineatus L.
Liolepisma laterale Say.
Eumeces quinquelineatus L.

SERPENTES:

Heterodon simus L.
Diadophis punctatus L.
Abastor erythrogrammus Daud.
Farancia abacura Holbr.
Coluber guttatus L.
Coluber quadrivittatus Holbr.
Zamenis constrictor L.
Zamenis flagelliformis Shaw.
Compsosoma corais couperii Holbr.
Pityophis melanoleucus Daud.
Ophibolus getulus getulus L.
Osceola doliata coccinea Schl.
Osceola elapsoidea Holbr.
Storeria dekayi Stor.
Natrix fasciata erythrogaster Shaw.
Natrix cyclopium D. & B.
Natrix taxispilotus Holbr.
Eutænia sirtalis sirtalis L.
Tantilla coronata B. & G.
Elaps fulvius L.
Systrurus miliarius L.
Crotalus adamanteus adamanteus L.

The total number of species of the Floridan subregion is as follows:

Peculiar species,	15
Species common to the Ocmulgian district,	1
Species common to the Louisianian district,	40
Species common to the West Indian region,	3
Little known species,	2
	61

VI. The Sonoran Subregion.

This subregion presents several natural divisions, as follows: I. The Lower Californian district, including only the region at the extremity of the peninsula of Lower California; II. The Chihuahuan district, embracing the State of Sonora, Mexico, the northern part of the Mexican Plateau, Arizona south of the San Francisco Mountains, most of the peninsula of Lower California, and most of New Mexico; III. The Basin district, embracing the Great Basin of Utah and Oregon, to Vernon, British Columbia; and IV. The Central district, which includes the high plains east of the Rocky Mountains, from Texas northward, excepting the river bottoms which cross it from west to east. This great subregion is bound together by the general distribution of numerous genera; but I do not know a single species which covers its entire area which is not found elsewhere. These define the districts.

The Lower Californian district is defined by the following fourteen species, which are restricted to it:

Hyla curta Cope.
Ctenosaura hemilopha Cope.
Uta thalassina Cope.
Uta nigricauda Cope.
Phyllodactylus unctus Cope.
Cnemidophorus maximus Cope.
Euchirotes diporus Cope.
Lichanura trivirgata Cope.
Zamenis aurigulus Cope.
Phyllorhynchus decurtatus Cope.
Pityophis vertebralis Blv.
Chilomeniscus stramineus Cope.
Tantilla planiceps Blv.
Crotalus enyo Cope.

The district shares with the Chihuahuan the following species:

Bufo punctatus B. & G.
Dipsosaurus dorsalis Hallow.
Crotaphytus wislizenii B. & G.
Callisaurus draconoides Blv.
Sauromalus ater Dum.
Uta stansburiana B. & G.
Uta ornata B. & G.
Sceloporus zosteromus Cope.
Phrynosoma coronatum Blv.
Phyllodactylus tuberculosus Wiegm.
Salvadora grahamiæ B. & G.
Ophilobolus getulus boylii B. & G.
Chilomeniscus fasciatus Cope.
Hypsiglena ochrorhynchus Cope.
Natrix valida Kenn.
Eutænia eques Reuss.
Trimorphodon lyrophanes Cope.
Crotalus adamanteus atrox B. & G.
Crotalus mitchellii Cope.

Species common to the Lower Californian district and the Western subregion (mostly to the Diegan district) are the following:

Hyla regilla B. & G.
Phrynosoma coronatum Blv.
Verticaria hyperythra Cope.
Gerrhonotus multicarinatus Blv.
Opibolus getulus boylii B. & G.
Opibolus getulus californiæ Blv.
Plethodon croceater Cope.

Total species of the Lower Californian district:

Peculiar to it,	14
Common to the Chihuahuan district,	18
Common to the Western subregion,	7
	39

Thirty-eight species, one being twice enumerated as common to the Chihuahuan district and Western region.

The Chihuahuan district possesses the following peculiar species:

BATRACHIA SALIENTIA:

Bufo alvarius Grd.
Hyla arenicolor Cope.

SAURIA:

Ctenosaura multispinis Cope.
Crotaphytus reticulatus Bd.
Callisaurus notatus Bd.
Callisaurus rufopunctatus Cope.
Callisaurus inornatus Cope.
Callisaurus scoparius Cope.
Uta symmetrica Bd.
Uta bicarinata Dum.
Uta graciosa Hallow.
Sceloporus clarkii B. & G.
Sceloporus couchii B. & G.
Sceloporus jarrovii Cope.
Sceloporus ornatus B. & G.
Phrynosoma solare Gray.
Anota modesta Gir.
Anota maccallii Hallow.
Heloderma suspectum Cope.
Gerrhonotus multifasciatus D. & B.
Cnemidophorus tessellatus Say.
Cnemidophorus inornatus B. & T.
Cnemidophorus octolineatus B. & S.
Cnemidophorus guttatus B. & G.
Eumeces guttulatus Hallow.

OPHIDIA:

Glauconia dissecta Cope.
Glauconia dulcis B. & G.
Glauconia humilis B. & G.
Lichanura roseofusca Cope.
Diadophis regalis regalis B. & G.
Heterodon nasicus kennerlyi Kenn.
Zamenis semilineatus Cope.
Coluber emoryi B. & G.
Rhinechis elegans Kenn.
Pityophis sayi sayi Schl.
Epiglottophis pleurostictus D. & B.
Ophibolus getulus splendidus B. & G.
Chionactis occipitalis Hallow.
Chilomeniscus ephippicus Cope.
Gyalopium canum Cope.
Eutænia megalops Kenn.
Eutænia elegans marciana B. & G.
Eutænia elegans dorsalis B. & G.
Eutænia augustirostris Kenn.
Eutænia nigrilatus Brown.
Eutænia rufopunctata Cope.
Eutænia multimaculata Cope.
Trimorphodon upsilon Cope.
Trimorphodon lambda Cope.
Trimorphodon wilkinsonii Cope.
Scolecophis æmulus Cope.
Elaps euryxanthus Kenn.
Crotalus molossus B. & G.
Crotalus scutulatus Kenn.
Crotalus lepidus Kenn.
Crotalus cerastes Hallow.

Fifty-eight species, disposed of as follows: Batrachia salientia, 2; Sauria, 25; Serpentes, 31. Three species of Testudnata are peculiar to this district, viz.: *Kinosternum henrici* Lec., *K. flavescens* Agass., *Xerobates agasizii* Cooper. This district possesses a larger number of peculiar species than any other in the Medicolumbian Region.

The Basin district has but few peculiar species. Its southern boundary may be regarded as the San Francisco Mountains in northern Arizona. The *Crotalus tigris* which is restricted to it has been shown by Merriam to inhabit only the mountains, and its northern limit is as yet unknown. The following are the species of the Great Basin:

BATRACHIA:

Amblystoma tigrinum Green.
Spea intermontana Cope.*
Rana draytonii onca Cope.*
Rana pipiens brachycephala Cope.*

SAURIA:

Crotaphytus collaris Say.†
Crotaphytus wislzenii B. & G.†
Uta stansburiana B. & G.†
Sceloporus biseriatus Hallow.†
Sceloporus graciosus B. & G.†
Sceloporus consobrinus B. & G.†
Phrynosoma douglassii ornatissimum Gird.†
Anota platyrhina Gird.†
Zamenis tæniatus Hallow.†
Pityophis sayi bellona B. & G.†
Chionactis episcopus isozonus Cope.*
Eutænia elegans vagrans B. & G.
Crotalus tigris B. & G.†
Crotalus confluentus lecontei Hallow.

The species and subspecies peculiar to the Basin district are marked with a star, and those found also in the Chihuahuan with a dagger.

The Central district possesses but few peculiar species. These with certain Chihuahuan species give it a distinctive character. There are also a few species which enter it from the Eastern subregion. These are marked with a dagger, while the peculiar forms are marked with a star.

URODELA:

Amblystoma tigrinum Green.

SALIENTIA:

Bufo cognatus Say.*
Spea hammondii bombifrons Cope.*

SERPENTES:

Heterodon nasicus nasicus B. & G.*
Ophibolus multistratus Kenn.*
Zamenis constrictor L.†
Eutænia radix B. & G.†
Eutænia sirtalis parietalis Say.
Eutænia elegans vagrans B. & G.
Crotalus confluentus confluentus Say.

SAURIA:

Crotaphytus collaris Say.
Holbrookia maculata B. & G.
Phrynosoma douglassii hernandesii Gir.*
Eumeces septentrionalis Bd.*
Eumeces multivirgatus Hallow.†
Eumeces obsoletus B. & G.

The species not marked with dagger or star are Chihuahuan, except *Eutænia elegans vagrans*, which is also found in the Basin district, *E. sirtalis parietalis*, which extends to the Pacific district, and the *Amblystoma tigrinum*, which is Medicolumbian throughout.

The total number of species of the Sonoran subregion is as follows:

Peculiar to the Chihuahuan district,	58
Common to Lower Californian and Chihuahuan districts,	19
Peculiar to the Lower Californian district,	14
Peculiar to the Basin district,	6
Common to the Basin and Chihuahuan,	8
Peculiar to the Central district,	8
Common to the Central and Chihuahuan,	3
Common to the Chihuahuan and Texan,	14
	126
Doubles emplois,	4
	122

VII. The Western Subregion.

This subregion presents two distinct modifications, a northern and a southern. The boundary between the two has not yet been defined; it represents the demarkation between the greater humidity of the north and the arid conditions of the south. The name of Diegan has been given by Mr. Van Denberg to the southern region; to the northern I propose to restrict the name Pacific, which I formerly used for the entire subregion, which had been previously named the Western by Baird. The Pacific district extends further south along the Sierra Nevada than in the San Joaquin Valley. Some of the forms of the Diegan district extend north to the latitude of San Francisco, but the majority of the species are restricted to more southern latitudes. How far the Diegan district extends on the Lower Californian Peninsula is uncertain. The separation from the Chihuahuan district is also undertermined, and the species of both districts mingle in some degree on their borders.

Species peculiar to the Diegan district are the following:

Batrachia:

Bufo columbiensis halophila B. & G.

Sauria:

Uta repens Van Denberg.
Uta mearnsii Stejneger.
Sceloporus orcuttii Stejneger.
Sceloporus vandenbergianus Cope.
Phrynosoma cerroënse Stejneger.
Anota goodei Stejneger.
Xantusia vigilis Bd.
Xantusia riversiana Cope.
Xantusia picta Cope.
Zablepsis henshawii Stejneger.
Amæbopsis gilbertii Van Denburg.
Verticaria sericea Van Denberg.
Cnemidophorus tessellatus multiscutatus Cope.
Cnemidophorus tessellatus rubidus Cope.
Anniella pulchra Gray.

Serpentes:

Lichanura orcuttii Stejn.
Diadophis amabilis amabilis B. & G.
Crotalus ruber Cope.

To these must be added the species already enumerated as common to the Diegan and Lower Californian districts, and the following list of species which occur also in the Chihuahuan district:

Crotaphytus wislizenii B. & G.
Callisaurus draconoides Blv.
Uta stansburiana B. & G.
Sceloporus biseriatus Hallow.
Lichanura roseofusca Cope.
Crotalus adamanteus atrox B. & G.

The following species are common to the Diegan and Pacific districts:

BATRACHIA:

Diemyctylus torosus Esch.
Hyla regilla B. & G.*

SAURIA:

Phrynosoma blainvillii Gray.
Gerrhonotus multicarinatus Blv.*
Gerrhonotus burnettii Gray.
Eumeces skiltonianus B. & G.

SERPENTES:

Charina bottæ Blv.
Zamenis lateralis Hallow.
Zamenis tæniatus Hallow.*
Pityophis catenifer Blv.
Ophibolus getulus boylii B. & G.*
Eutænia elegans couchii Kenn.*
Eutænia infernalis infernalis Blv.
Crotalus confluentus lucifer B. & G.

These species are then characteristic of the Western subregion as a whole, except those marked with a star, which occur elsewhere.

The Pacific district is especially characterized by certain genera and species of Batrachia. No certainly known genus of scaled reptiles, and a limited number of species and subspecies are peculiar to it. Conspicuous among these are the species of Eutænia, which display great variety, while they are but sparsely represented in the Diegan district. The peculiar species are as follows:

URODELA:

Amblystoma macrodactylum Baird.
Amblystoma epixanthum Cope.
Chondrotus paroticus Baird.
Chondrotus decorticatus Cope.
Chondrotus aterrimus Cope.
Chondrotus tenebrosus B. & G.
Batrachoseps caudatus Cope.
Batrachoseps attenuatus Esch.
Plethodon intermedius Bd.
Plethodon oregonensis Gird.
Autodax lugubris Hallow.
Autodax iëcanus Cope.
Autodax ferreus Cope.
Diemyctylus torosus Esch.
Bufo columbiensis columbiensis B. & G.
Spea hammondii hammondii Bd.
Rana temporaria pretiosa Bd.
Rana cantabridgensis latiremis Cope.
Rana agilis aurora B. & G.
Rana draytonii Baird.
Rana boylii Baird.

SAURIA:

Sceloporus undulatus occidentalis Bd.
Phrynosoma douglassii douglassii Bell.
Gerrhonotus principis B. & G.
Cnemidophorus septemvittatus Cope.

SERPENTES:

Diadophis amabilis pulchellus B. & G.
Zamenis constrictor vetustus B. & G.
Contia mitis B. & G.
Eutænia elegans elegans B. & G.
Eutænia elegans lineolata Cope.
Eutænia elegans ordinoides B. & G.
Eutænia infernalis vidua Cope.
Eutænia sirtalis parietalis Say.
Eutænia sirtalis trilineata Cope.
Eutænia sirtalis pickeringii B. & G.
Eutænia sirtalis tetratænia Cope.
Eutænia sirtalis concinna Hallow.
Eutænia biscutata Cope.
Eutænia leptocephala B. & G.

There are therefore peculiar to the Pacific district eighteen species and three subspecies of Batrachia (two species found in the Holarctic region represented by subspecies, and one species

from the Canadian); two species and two subspecies of lizards; and three species and eleven subspecies of snakes.

We have of species and subspecies of the Western subregion the following synopsis:

Peculiar to the Diegan district,	19
Common to the Diegan and Chihuahuan,	6
Common to the Diegan and Pacific,	11
Peculiar to the Pacific,	39
	75

VIII. The Toltecan Subregion.

This subregion includes three districts which possess characteristic species, and which differ in climate. The Austroriental is a humid region with abundant rains and fogs, and includes the eastern face and slope of the central plateau, with the mountain elevations, including parts of the States of Puebla, Vera Cruz, Hidalgo and San Louis Potosi. It is cut off to the north from the Austroriparian subregion by an interval in the States of Nuevo Leon and Tamaulipas. The middle or Austrocentral district includes the valleys of Mexico and Toluca, and the region northward to the edge of the Sonoran subregion, including the State of Guanajuato, and perhaps further north. The climate of this district is much less humid than that of the Austroriental district. The Austroccidental district includes the high lands of Oaxaca, Guerrero, Michoacan and Jalisco. It is the most arid of the three divisions, and extends furthest to the south and west.

The northern boundary of the Toltecan district is not yet determinable; hence it is not possible to state whether species from the States of Durango and Zacatecas, such as *Eutænia angustirostris*, should be referred to it or not. A small collection made by Wilkinson in southern Chihauhua at Batopilas[1] has the character of the Chihuahuan fauna, with the following species not otherwise found in it:

Anolis nebulosus Wiegm.
Uta bicarinata Dum.
Scolecophis æmulus Cope.

[1] Cope, Proceeds. Amer. Philosoph. Soc., 1879, p. 261.

The humid and dry districts of the Toltecan subregion repeat *in petto* the differences between the Austroriparian and Sonoran subregions. The Austroriental district is distinguished by the larger number of batrachian genera and species, and of certain genera of Crotalidæ. It also includes some genera which may be regarded as immigrants from the Central American region of the Neotropical Realm.

The characteristic species of the *Austrocentral district* are[2]:

BATRACHIA URODELA:

Siredon mexicanum Shaw.
Amblystoma tigrinum Green.

BATRACHIA SALIENTIA:

Bufo compactilis Wiegm.
Bufo intermedius Gthr.
Spea multiplicata Cope.
Spea hammondii Bd.
Hyla eximia Bd.
Hyla arenicolor Cope.
Rana montezumae Bd.

TESTUDINATA:

Kinosternum pennsilvanicum.
Onychotria mexicana Gray.

SAURIA:

Phrynosoma orbiculare Wiegm.
Sceloporus scalaris Wiegm.
Sceloporus microlepidotus Wiegm.
Sceloporus torquatus Green.
Sceloporus minor Cope.
Sceloporus melanogaster Cope.
Barissia imbricata Wiegm.
Cnemidophorus guttatus B. & G.
Eumeces brevirostris Gthr.

SERPENTES:

Conopsis nasus Gthr.
Toluca lineata Kenn.
Chionactis varians Jan.
Salvadora bairdii Jan.
Epiglottophis pleurostictus D. & B.
Hemigenius variabilis Dugés.
Natrix storerioides Cope.
Eutænia macrostemma Kenn.
Eutænia eques Reuss.
Eutænia pulchrilatus Cope.
Eutænia scaliger Jan.
Eutænia melanogaster Wiegm.
Tantilla bocourtii Gthr.
Tantilla calamarina Cope.
Crotalus basiliscus Cope.
Crotalus polystictus Cope.

[2] For the exact habitat of several of these I am indebted to the important papers of Dr. A. Dugés, in La Naturaleza, 1888, p. 97, and 1896 p. 3.

Of these species the following occur in the Chihuahuan district:

Amblystoma tigrinum Green.
Spea hammondii Bd.
Hyla arenicolor Cope.
Sceloporus scalaris Wiegm.
Sceloporus microlepidotus Wiegm.
Chemidophorus guttatus B. & G.
Epiglottophis pleurostictus D. & B.
Eutænia macrostemma Kenn.
Eutænia eques Reuss.

The Austroriental district includes the mountainous region which bounds the Mexican Plateau on the east, from some part of the State of Puebla to a point to the north not yet ascertained. It is probably separated by a considerable interval from the Austroriparian in the States of Tamaulipas and Nuevo Leon. Its climate is moist, and vegetation is abundant, and of principally Medicolumbian type. Various peculiar species of Acer, Platanus, Quercus, Andromeda and other forms are abundant. The Batrachian and Reptilian species are the following:[3]

BATRACHIA URODELA:

Spelerpes chiropterus Cope.
Spelerpes leprosus Cope.
Spelerpes cephalicus Cope.
Spelerpes orizabensis Blatchley.
Spelerpes gibbicaudus Blatchley.
Oedipina lineola Cope.
Thorius penuatulus Cope.

BATRACHIA SALIENTIA:

Hyla gracilipes Cope.
Hyla miotympanum Cope.
Hyla bistincta Cope.
Smilisca baudinii D. &. B.

SAURIA:

Sceloporus variabilis Wiegm.
Sceloporus æneus Wiegm.
Sceloporus microlepidotus Wiegm.
Phrynosoma orbiculare Wiegm.
Phrynosoma taurus Dugés.
Barissia imbricata Wiegm.
Barissia antauges Cope.
Gerrhonotus gramineus Cope.
Gerrhonotus tæniatus Wiegm.
Gerrhonotus liocephalus Wiegm.

[3] For a knowledge of the distribution of many of these species I am indebted to Francois Sumichrast, in Archives des Sciences, in Bibliotheque Universelle, 1873, p. 233, and *in litteris*.

Celestus enneagrammus Cope.
Liolepisma laterale Say.
Anelytropsis papillosus Cope.

SERPENTES:

Atractus latifrontalis Garm.
Ficimia olivacea Gray.
Epiglottophis lineaticollis Cope.
Osceola doliata polyzona Cope.
Ninia diademata B. & G.
Storeria dekayi Stor.
Storeria occipitomaculata Holbr.
Rhadinæa vittata Jan.
Rhadinæa decorata Gthr.
Eutænia sumichrastii Cope.
Eutænia chrysocephala Cope.
Eutænia pulchrilatus Cope.
Eutænia scalaris Cope.
Eutænia phenax Cope.
Sibon frenatum Cope.
Sibon personatum Cope.
Sibon albofuscum Lac.
Bothriechis mexicanus D. & B.
Ophryacus undulatus Jan.
Systrurus ravus Cope.
Crotalus triseriatus Wagl.

Of all the above species the following are found also in the Austrocentral district:

Barissia imbricata Wiegm.
Sceloporus variabilis Wiegm.
Sceloporus microlepidotus Wiegm.
Phrynosoma orbiculare Wiegm.
Eutænia pulchrilatus Cope.

Species found in the Austroriparian subregion:

Liolepisma laterale Say.
Storeria dekayi Stor.
Storeria occiptomaculata Holbr.

To the Austroriental list might be added *Spelerpes bellii* Gray, which is stated by Sumichrast to inhabit also the Tierra Caliente; and *Anolis nannodes* Cope, which the same authority says ranges from the Tierra Caliente into the Alpine district. The water-snake *Natrix rhombifera* Hallow. may occur in the Austroriental district, but this needs confirmation.

The *Austroccidental district* is inhabited by a number of peculiar species, together with some which occur in the other two districts of the Toltecan subregion. One peculiarity of this district is the poverty in Batrachia and the absence of Urodela. The peculiar species are the following:

BATRCHIA ANURA:

Leptodactylus melanonotus Hallow.
Hypopachus variolosus Cope.

SAURIA:

Sceloporus siniferus Cope.
Sceloporus horridus Wiegm.
Sceloporus rubriventris Gthr.
Sceloporus pyrrhocephalus Cope.
Sceloporus omiltemanus Gthr.
Sceloporus dugesii Boc.
Sceloporus bullerii Boul.
Sceloporus heterolepis Boul.
Cnemidophorus deppei lineatissimus Cope.
Eumeces callicephalus Boc.

SERPENTES:

Pseudoficimia frontalis Cope.
Sympholis lippiens Cope.
Atractus omiltemanus Gthr.
Adelophis copei Dugés.
Rhadinæa laureata Gthr.
Eutænia godmanii Gthr.
Chionactis michoacanensis Dugés.
Coniophanes lateritius Cope.
Conophis vittatus Pet.
Himantodes gemmistratus latistratus Cope.
Sibon personatum Cope.
Manolepis nasutus Cope.

Of the above species there are found in the Tierra Caliente:

Sceloporus siniferus Cope.
Sceloporus horridus Wiegm.
Sceloporus pyrrhocephalus Cope.
Conophis vittatus Pet.
Sibon personatum Cope.
Manolepis nasutus Cope.

And in the region south to Costa Rica:

Hypopachus variolosus Cope.
Himantodes gemmistratus Cope.

The Austroccidental district shares with the Austrocentral the following:

BATRACHIA ANURA:

Bufo compactilis Wiegm.
Hyla eximia Bd.
Rana pipiens austricola Cope.

SAURIA:

Phyllodactylus tuberculosus Wiegm.
Uta bicarinata Dum.
Barissia imbricata Wiegm.

Chemidophorus guttatus B. & G.
Sceloporus scalaris Wiegm.
Phrynosoma orbiculare Wiegm.
Anolis nebulosus Wiegm.

SERPENTES:

Drymobius margaritiferus Schl.
Diadophis lætus Cope.
Osceola doliata polyzona Cope.
Hemigenius variabilis Dugés.
Natrix storerioides Cope.
Eutænia eques Reuss.
Eutænia melanogaster Wiegm.
Epiglottophis pleurostictus D. & B.
Tantilla calamarina Cope.
Trimorphodon biscutatus D. & B.
Trimorphodon upsilon Cope.
Crotalus triseriatus Wagl.
Crotalus polystictus Cope.
Crotalus basiliscus Cope.

A number of species inhabit the Austroccidental and Austroriental districts, passing to the southward of the Austrocentral, at least so far as present information extends. These are the following:

BATRACHIA ANURA:

Smilisca baudinii D. & B.

SAURIA:

Sceloporus torquatus Green.
Phrynosoma taurus Dugés.
Gerrhonotus oaxacæ Gthr.

SERPENTES:

Rhadinæa vittata Jan.
Eutænia chrysocephala Cope.
Coniophanes proterops Cope.
Ophryacus undulatus Jan.
Crotalus triseriatus Wagl.

The species of the Toltecan subregion are as follows:

Austroriental district,	44
Austrocentral district,	36
Austroccidental district,	24
	73
Doubles emplois,	2
	71

VIII. Recapitulation.

The number of species of Reptilia Squamata of the Medicolumbian region is as follows. The species of Batrachia have been already enumerated in my book on that class.[4]

Superfamilies.	Families.	Genera.	Species.
	SAURIA.		
Pachyglossa	Iguanidæ	12	79
Nyctisaura	Gecconidæ	2	2
	Eublepharidæ	1	1
Helodermatoidea	Helodermidæ	1	1
Diploglossa	Anguidæ	4	17
Leptoglossa	Tiidæ	2	11
	Xantusiidæ	3	5
	Scincidæ	2	20
	Anelytropsidæ	1	1
Annielloidea	Anniellidæ	1	2
Annulati	Euchirotidæ	1	1
	Amphisbænidæ	1	1
	Total Sauria	31	141
	SERPENTES.		
Catodonta	Glauconiidæ	1	3
Colubroidea	Boidæ	1	3
	Charinidæ	1	2
	Colubridæ	26	133
	Dipsadidæ	10	19
	Elapidæ	1	3
Solenoglypha	Crotalidæ	5	25
	Total Serpentes	45	188
	Sauria	31	141
Total Squamata		76	329

[4] The Batrachia of North America, Bulletin of the U. S. Natl. Museum, No. 34, 1889, p. 451. The species of the Toltecan subregion are mostly omitted from this book.

EDITOR'S TABLE.

It is difficult to eradicate from scientific literature a name or word which has become current, even after it has been found to be an expression of ignorance or error. Thus some names introduced into Zoology die hard. It is perfectly well-known that the grouping of forms named by Cuvier Pachydermata, is entirely unnatural, and the appropriate positon of all of its contents has been exactly determined; yet the word occasionally crops up still in the literature. The supposed primary divisions of fishes Ganoidei and Teleostei, have a still more vigorous vitality, although it is perfectly clear that there is no use for either term. The supposed Ganoid division is thoroughly heterogeneous, its contents forming with the Teleostei a more comprehensive division, the Teleostomi of Owen, which naturally falls into several primary divisions three of which were included in the Ganoidei by Agassiz and Müller. Perhaps the most pestilent pretender of the list, is the word Amphibia, which is so frequently used instead of the proper name of the class Batrachia. The name Amphibia was originally applied to a combination of the Reptilia and Batrachia, before the fundamental differences between the two were known. When the Batrachia were first separated from the Reptilia, the new name was naturally applied to the new division, and the name Amphibia would have been more applicable to the larger division of its former self i. e. the Reptilia. As, however, its definition accorded with neither the Reptilia nor Batrachia, it was not used for either, nor was it introduced to take the place of Batrachia with a definition, until a few years ago by Huxley. This was done in defiance of the universal usage of naturalists at the time, and probably in ignorance of the real state of the case, since it frequently happens that men engaged in the real work of biological science, find questions of names irksome and stupid. Nevertheless it is a distinct advantage always to have but one name for one thing; and that name should be the oldest which was applied to the thing in question as determined by the definition given. Applying this principle, the name Batrachia has a quarter century priority over Amphibia.

In the April, 1896 number of this journal (p. 292) we published what purported to be a review of a work by Wachsmuth and Springer, which was signed by one of our frequent contributors. In a foot note the work is stated to have been published in 1895. We have learned

from leading authorities on the subject of the work, (the Crinoidea), that it was not published at the time the review was issued, nor it is yet published. We make this statement, since it is important that the date of publication of all books, especially scientific books, should be correctly ascertained and reported, and because we desire to prevent any confusion as to the date of this particular publication which might arise from our having published the review in question. As is usual with periodicals, we assume no responsibility for articles published in the NATURALIST unless they are anonymous.

The dates of publication of the numbers of the AMERICAN NATURALIST during the years 1895, and 1896 are as follows: for 1895; Jan., Jan. 15th; Feb., Feb. 14th; March, Mch. 6th; April, Apl. 9th; May, May, 13th; June, June 3d; July, July 9th; August, July 31st; Sept., Aug. 28th; Oct., Sept. 26th; Nov., Oct. 29th; Dec., Dec. 6th.

For 1896; Jan., Dec. 31st, 1895; Feb., Jan. 30th; March, Mch. 9th; April, Apl. 2d; May, May 2d; June, June 3d; July, July 2d; August, Aug. 6th; Sept., Sept. 9th; October, Oct. 3d; Nov., Nov. 2d; Dec., Dec. 5.

RECENT LITERATURE.

Gregory's Plant Anatomy.[1]—Among the host of botanical textbooks that are constantly appearing, it is a pleasure to welcome one that is a contribution to certain departments of botanical literature, rather than a mere exposition of the laboratory and lecture methods, good, bad, and chiefly indifferent, of the author. While it is to be assumed that American investigations in histology and in cytology have not been lacking during these past few years, the fact remains that they have not as yet resulted in an increase of literature upon these subjects.

While there can be no doubt that the tide is setting steadily and strongly in the direction of higher things in cisatlantic botany, this is as yet a premonition rather than a fact, and the few texts leading toward this are to be regarded as pioneers and valued as such. These books are divisible into two classes, and in evaluating them, it is necessary to measure them by a proper standard. Thus, a book which purports to be a textbook should not be criticized because it does not manifest

[1] Elements of Plant Anatomy, by Emily L. Gregory, Ph. D. Professor of Botany in Barnard College. Ginn & Co., Boston, 1895, pp. VIII, 148. 8vo.

the depth and comprehensiveness of an exhaustive treatise, nor should an elaborate work on original investigation be supposed to cover the details of elementary science.

The present book is intended to serve as an introduction to the elements of phytotomy. This purpose is effected more than ordinarily well. It is no mean task to distinguish between the relevant and the irrelevant, between the essential and the non-essential in the construction of an elementary text. In these very points, the author has been particularly happy, and deserves congratulation upon the coherency and the coordination manifested in the text.

A striking feature of the book is its prevailing clearness. Many otherwise well written and helpful text-books are marred by the fact that too much is written between the lines, a thing deplorable in any scientific writing, but especially so in an elementary one. The author has succeeded, however, not only in establishing delightful perspicacity of style, but also in maintaining it throughout the work. In consequence, the beginner may find here a text which presents in a remarkably easily assimilated condition those rudiments of plant anatomy which should serve as a foundation for advanced botanical study in all lines.

The merits of the book are many and obvious, and warrant passing its few defects in silence. Its inspiration is readily recognizable as of the German school, an additional point in its favor were it not for a prefatory remark to which the reviewer must enter serious objection. The author states that "it is quite certain that the measure of our progress in any science may be found in our ability to adapt the thought and experience of other nations to our special needs and resources," a statement of such a very peculiar nature that comment is superfluous.

The book is divided into two parts, the first of which treats of cytology, or, as the author terms it, the anatomy of the cell. Under this, the first chapter treats of the cell as a unit, the second and third present the subjects of cell-wall and cell-contents in their modern aspects. The second part discusses the anatomy of tissues, first generally, and then more specially, with reference to the thoroughly antiquated divisions, Thallophytes and Cormophytes. The last chapter, the irrelevancy of which is excused by its importance, is devoted to an exposition of the secondary growth of stems and roots.—FREDERIC E. CLEMENTS.

Boulenger's Catalogue of Snakes in the British Museum.[1] —In this work we have a manual of Ophiology in which the subject is

[1] Catalogue of the Snakes in the British Museum. Vol. I, 1893; vol. II, 1894; vol. III, 1896. By G. A. Boulenger, F. R. S.

as nearly as possible brought up to date. The especial advantage of being the work of the Keeper of the largest collection of Ophidians in the world, makes this catalogue of especial value to all students. The author informs us that there are known 1639 species of snakes, of which 1327 are represented in the collection of the British Museum by 11092 specimens.

A good deal of valuable new osteological work enters into the systematic, which will be at once recognized by specialists. Thus the determination of the forms which have elongate hypapophyses throughout the vertebral column is here made for the first time, and the discovery that all the Colubridæ of Madagascar have the prolonged series of hypapophyses, is one of the notable announcements of the work. The peculiar pterygoids of the Amblycephalidæ are the author's discovery, as are also the split ectopterygoids of Dispholidus, etc., and the confluent optic foramina of the Psammophiinæ[2] The labor of specific determination of over 11000 specimens, in an order where variation is often conspicuous, is, however, the great feature of such a work as this, and even the approximately complete form in which it is now presented, is a monument to the industry and acumen of its author, and a service rendered to science by the British Museum which will always remain.

There are, however, some spots on the face of this illuminating ing production. The labor of determining the true limits of variable species has in a good many instances, it seems to us, proven too much for the patience of the author, and he has resorted to the convenient method of "lumping" too often. He has given up a valuable feature of the older catalogues, the list of doubtful species. In the present work all published species are either good or bad, whether the author has had the requisite opportunity of determining their true status or not. Thus it has happened in not a few instances that names relegated to the synonymy in the body of the work are reintroduced in the Addenda as belonging to good species. Had the author the material it is probable that a good many others would have been recognized before the final issue of the Catalogue. The author has been especially unfortunate in his treatment of North American species, and the student of North American Ophiology will not find his knowledge of this subject increased by this publication. Some of the species studies are on the other hand very thorough, as for instance the genera Vipera and Naja. The revision of the synonymy of both the older and later European authors is a service for which all herpetologists will be grateful.

[2] I mention here that the genera Malpolum, Psammophis, Mimophis and Rhamphiophis have no protrusible male intromittent organ. For this reason I propose to arrange them as a special subfamily, the Psammophiinæ.

The primary divisions of the Ophidia (or Serpentes as they should be called) adopted, are nine families, which have very different values. These can be associated in superfamilies of approximately equal value, but this Dr. Boulenger has not done, but has contented himself with giving an analytical table (pp. 1–2), where some of the characters of these superfamilies are pointed out, in the dichotomous order, which does not express relative value. Many groups usually regarded as families are not recognized, as for instance the Najidæ and Dipsadidæ, which are included in the Colubridæ. In a phylogenetic table the interesting suggestion is made that the Solenoglyphous snakes are derived from the Opisthoglyphous, and not from the Proteroglyphous.

In seeking for generic characters the dentition has been closely examined. The value of dental characters has been thoroughly tested, and the result is valuable to the student, although we do not always agree with the use made of the information in the Catalogue. The author does not adopt the characters used by Duméril and Bibron in many instances, for good reasons, but he introduces others of his own which are no better, as the numbers and in some cases the relative lengths of the teeth. In practice it is often impossible to determine whether teeth are of equal length or a little longer at one or the other end of the jaw; nor is the number of the teeth in the jaws precisely definitive of anything but species, as can be readily seen from the results recorded in the present work. The division or union of the anal plate and urosteges, is generally rejected as a character, although its value is testified to by the uniform use made of it by ophiologists. In fact the generic definitions are based on no uniform principle, and the author seems to have been possessed at times with the idea that it were an especial merit to differ as much as possible from his predecessors.

One result of the study of this work will be to prove to ophiologists that it is desirable to become acquainted with new characters of definitive value before we can have the true system of the snakes. An important addition to our knowledge in this direction, i. e. of the characters of the hemipenis and of the lungs, came too late to be incorporated in the present work.—E. D. Cope.

Nuttall's Handbook of Birds.[1]—A new edition, with important additions, and a series of more than one hundred colored illustrations.

[1] *A Popular Handbook of the Ornithology of Eastern North America.*—By Thomas Nuttall. Revised and annotated by Montague Chamberlain. Vol. I, *Land Birds.* Vol. II *Game and Water Birds* Second edition, with corrections and additions Illustrated with one hundred and seventy-two figures, two colored

This favorite work, easily understood, handy, and popular, including all of Nuttall's delightful descriptions of bird-life, which was some time since fully annotated by Montague Chamberlain, who added the birds not known in Nuttall's time, will be found more useful and valuable than ever before, Mr. Chamberlain having again gone over the work with the greatest care, bringing the information down to date.

Colored representations of the birds being desirable for amateurs and students, a series of twenty plates, containing one hundred and ten figures of birds, has been added to the present edition. The drawings have been mostly copied from those of Wilson, and may be relied on for accuracy, although in some instances the tints do not come up to the brilliancy of Nature. We recommend the book as the one for the family, where the strictly scientific side of ornithology is not the chief desideratum. We mean by this that the work is not devoted to the anatomy and physiology of birds, but is one by which the species may be identified, and where descriptions of their habits and geographical range may be found; all set forth in admirable style.

Education of the Central Nervous System.[2]—This book is an endeavor to apply the most recent results of psychology and brain physiology to the theory of education. The author quotes from Donaldson and other well-known writers on the topography of the brain and localization of functions. In view of the close connection between cerebral development and mental capacity, he advocates an education which shall develop all parts of the brain to the greatest possible extent. He recommends especially that children be trained to distinguish every shade of sensation-difference, and to recall in vivid images the objects of every kind which they have experienced; if such training be begun early in life, the brain cells are better developed, and in after life our mental images are more numerous and more definite.

Unfortunately the book is limited almost exclusively to a discussion of sensation and memory, leaving out of account entirely the higher rational processes. It becomes an appeal for an education which is fundamentally æsthetic and literary, as distinguished from scientific. Book learning for children is decried, and teachers are urged to take their pupils out into the woods and fields, and have them learn from

frontispieces, and twenty colored plates, containing one hundred and ten figures of the most important land and water birds. 2 vols. Crown 8vo. Cloth, extra, gilt top, $7.50 *uet;* half crushed Levant morrocco, extra, gilt top, $13.50 *net.*—Little, Brown & Co., Publishers, 254 Washington Sreet, Boston.

[2] The Education of the Central Nervous System, by R. P. Halleck. New York, The Macmillan Co., 66 Fifth Ave. 1896. Pp. xii, 258; price $1.

nature herself. This was the education, the author thinks, which made Shakespere really great. The study of nature is certainly of value, and the author's recommendations, together with the practical exercises in sense-training which he gives, will doubtless be an aid to this culture. But in these days of the supremacy of science, it is far more important to begin early to lay the foundations of habits of correct scientific thinking. The possession of clear and vivid mental imagery is a factor in correct thinking, of course; but unless accompanied by the logical treatment of ideas it is quite as likely to lead us in the wrong as in the right direction.

As a manual on the education of the central nervous system Mr. Halleck's work is very incomplete; it must be supplemented in several directions, and notably by a considerable amount of that very "book-learning" which the author treats so lightly. The treatment of motor education is inadequate, being confined to a single short chapter at the end of the book. By way of minor criticism, we may notice the author's fondness for repeating the same illustrations (e. g., pp. 82, 248). Some of his deductions are based on very inadequate data (e. g., p. 64); but this is rather the fault of his authorities. His list of great men who began to show talent at an early age, though large, calls to mind so many exceptions as to throw considerable doubt on the position which it seeks to establish.

The chapter entitled: "How Shakspere's Senses were Trained," is interesting to the student of literature, though somewhat too detailed. Throughout the book there is a wealth of quotations from Shakespere, Milton, and other writers, which add to its literary finish, if they do not improve its scientific quality.—H. C. W.

Lydekker on the Geographical History of Mammalia.[1]—I have already referred to this work in the last number of the NATURALIST in a paper on the Geographical Distribution of Batrachia and Reptilia of North America. I then pointed out that the author adopts the three Geographical realms of Huxley with the reasons why in my opinion the Ethiopian should constitute a fourth Realm. The divisions of the Notogæic Realm of Lydekker's system, are the Australian, Polynesian, Hawaiian and Austromalayan. The Neogæic realm has a sole region, the Neotropical. The Arctogæic is divided into the Malagasy, the Ethiopian, the Oriental, the Holarctic, and the Sonoran. Having otherwise disposed of the Ethiopian and its subdivision the Malagasy,

[1] The Geographical History of Mammals; by R. Lydekker A. B., F. R. S., V. P. G. S., etc. Cambridge University Press, 1896. 8vo. pp. 400.

I adopted the three remaining regions, the Oriental, the Holarctic, and the Medicolumbian; the last name being derived from Blanford, and used as a substitute for Sonoran, which have been previously used for a subdivision.

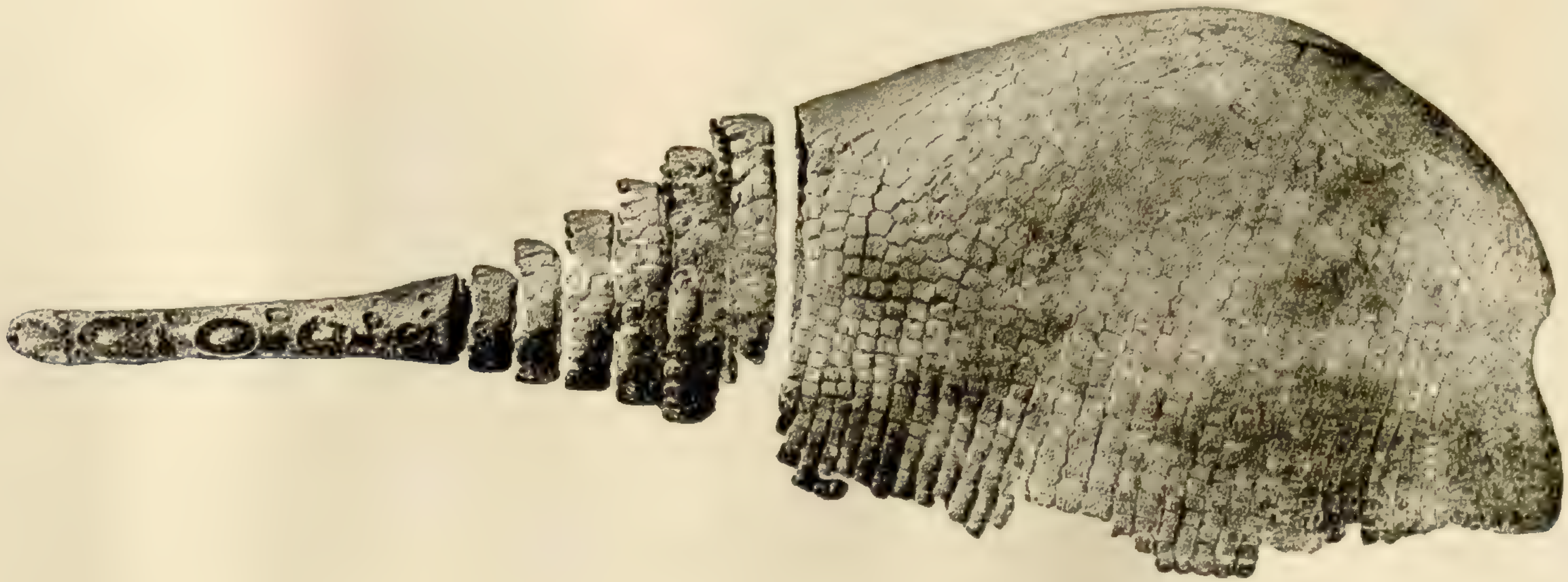
This work is a magazine of information on the subject of which it treats, and a unique feature is the large amount of reference to the facts of paleontology. This increases the value of the book to the general reader, but cannot be said to be germane to its main object. The introduction of the extinct forms of life necessarily changes the aspect of the faunal lists of a country to a marked degree, nowhere more so than in the Arctogæan Realm. Each geological period had in fact its own geographical distribution of forms, and when all are discovered a series of books on geographical distribution in each period might be written, each different from every other one.

Plagiaulax minor from the English Wealden; much enlarged.

Manis tricuspis West Africa.

The well-known familiarity of the author of this book with both Mammalian zoölogy and paleontology, gives it a value which no similar book possesses; and its compact form and fulness of illustration

PLATE XXVIII.



External skeleton of *Panochthus tuberculatus* from Argentina; much reduced.

make it especially convenient for the traveller who reads as he goes. The author writes clear and direct English, and correct classical orthography. His systematic of the Mammalia given on p. 11 is uncritical, though it includes most of the groups brought to light by paleontology. More detailed classification in later chapters elucidates the subject further.

The accompanying three illustrations give a good idea of their their general character.—E. D. Cope.

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General Notes.

PETROGRAPHY.[1]

The Sioux Quartzite of Iowa.—The Sioux quartzite has long been known as the oldest sedimentary rock in Iowa. It has recently been studied by Beyer.[2] It is a white or red vitreous rock with which is associated as its upper extension a series of mottled reddish or purplish-black slates. The quartzites present the usual aspects of indurated sandstones. The constituent quartz grains are rich in 'quartz-needles' which can be traced directly into rutile spicules. The slates are arenaceous. They exhibit no traces of slaty cleavage, though in some cases their quartz grains and micaceous constituents are distorted in such a way as to testify to a horizontal movement in the rock mass containing them. All the slates are mottled by spheroidal masses of a lighter color than the body of the rock. These masses are spheroidal with the longer dimensions of the spheroids in the bedding planes of the shale. Their lighter color is supposed to be due to the removal of iron from those portions of the rock they occupy. Associated with the quartzites is a great mass of olivine diabase consisting of a coarse grained aggregate of labradorite and oligoclase zonally intergrown, olivine, augite, biotite, hornblende, apatite and magnetite. Most specimens are much altered, the components having been changed into the usual secondary substances common to diabase. In structure the rock varies from the ophitic, in which the plagioclase is older than the augite, to the gabbroitic, in which the augite is the older mineral. An analysis gave:

SiO_2	TiO_2	Fe_2O_3	FeO	Al_2O_3	CaO	MgO	K_2O	Na_2O	H_2O	P_2O_5	Total
42·85	tr	13.66		20.23	6.85	3.42	1.90	5.78	.88	tr	= 100.57

The Peridotites of North Carolina.—In connection with a discussion of the occurrence and origin of corundum in North Carolina, Lewis[3] gives us an interesting account of the basic rocks associated with the gneisses in that portion of the Appalachian belt included within the limits of the State. These basic rocks, consisting mainly of

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Iowa Geol. Survey, Vol. VI, p. 69.

[3] Bull. No. 11, North Carolina Geol. Survey, 1896.

peridotites, occur in small lenticular masses or in narrow strips, which are always enveloped in a sheet of schistose talc or chlorite, and thus are never in direct contact with the gneisses through which they are believed to cut. They are classed as peridotites, pyroxenites and amphibolites, the former being the most common. The peridotites present several types in each occurrence, all merging into one another and forming a single geological unit. The principal types of the peridotites are dunite, harzburgite, amphibole-picrite and forellenstein. All are massive, as a rule, though exceptions are noted. The dunite is composed of olivine grains, octahedrons and rounded grains of picotite and chromite, plates of enstatite, prisms of light green hornblende and various alteration products of these, the most common being serpentine tremolite and chlorite. The Harzburgite and the other peridotites present no unusual features. They appear to be transition phases between the dunite and the various pyroxenites among which are recognized two types, an enstatite rock and websterite. The enstatite rock is made up almost exclusively of enstatite or bronzite and its alteration product talc. An analysis of the enstatite gave:

SiO_2	Al_2O_3	FeO	CaO	MgO	MnO	H_2O	Total
51.64	.12	9.28	.45	31.93	.56	5.45	99.43

The amphibolites are composed chiefly of amphibole. The most important type is composed of grass-green hornblende, anorthite and more or less corundum. The rock is fine grained and it is usually gneissic, although occasionally massive. Transitions through forellenstein into dunite were observed, although the distribution of the rock suggests its occurrence in a system of dykes cutting the latter rock. The hornblende has the following composition:

SiO_2	Al_2O_3	Cr_2O_3	FeO	NiO	MgO	CaO	NaO	K_2O	H_2O	Total
45.14	17.59	.79	3.45	.21	16.69	12.51	2.25	.36	1.34	= 100.33

Genth called the mineral smaragdite. Dana regards it as edenite. In addition to the rocks mentioned above, there are also present in the region massive serpenite, which was unquestionably derived from dunite, talc-schists, and soapstones derived from enstatite rocks and chlorite schists.

In a second paper[4] the same author gives his reasons for considering these rocks as eruptive in origin.

Shales and Slates from Wales.—Hutchins[5] continues his studies of clays, shales and slates by an investigation of the nature of

[4] Elisha Mitchell Sci. Soc. Jour., Pt. II, 1895, p. 24.

[5] Geol. Mag., Vol. III, 1896, p.

shales taken from some of the deepest coal mines in Wales. The chemical composition of the particular shale analyzed does not differ materially from that of some of the carboniferous shales from other coal fields. Physically the deeper shales are not much more compact than hard clays. The author reviews the results of his observations on shales and slates. He states that what takes place in a rock during its progress from clay to shale, is the development and crystallization of muscovitic mica and the production of chlorite. He also calls attention to the fact that dynamic metamorphism is made to explain many phenomena connected with the crystallization of slates, that are capable of being explained better by static metamorphism. The spots of many contact rocks are now thought to be secretions from a mineralizing solution, depositing in these spherical forms material collected from the rock body. By crystallization the spots pass over into cordierite, biotite or staurolite crystals.

Notes.—Cushing[6] declares that in addition to the rocks described by Kemp from the eastern Adirondacks there is a system of diabase dykes, which are older than the monchiquites and camptonites of the district.

By melting certain rock powders in the presence of reagents Schmutz[7] has obtained aggregates of minerals which in most cases are very different from those composing the original rocks. Eklogite fused in the presence of calcium and sodium fluride yielded a mass of meionite, plagioclase and glass; leucitite with calcium chloride gave a mass composed of a glassy groundmass and plagioclase; with the addition of sodium fluride and potassium silico-fluride it yielded scapolite, mica, magnetite; with sodium chloride it produced augite, scapolite and magnetite and a glass matrix. Granite fused with magnesium and calcium chlorides and sodium fluoride gave andesine and olivine in a groundmass containing augite. Other rocks treated with other reagents gave analagous results.

As the result of a series of experiments made with the view of discovering a medium with a very high specific gravity that will not attack sulphides, Retgers[8] finds that the acetate and the mixed nitrate and acetate of thallium are both neutral toward sulphides. The former is available for separating minerals with a density below 3.9, and the latter those with a density below 4.5.

[6] Trans. N. Y. Acad. Sci., XV, 1896, p. 249.
[7] Neues Jahrb. f. Min., etc., 1896, I, p. 211.
[8] Neues Jahrb. f. Min., etc., 1896, I, p. 213.

In an article in the *Neues Jahrbuch* Bauer[9] gives a German transcription of his article[10] on the rocks associated with the jadeite of Turmaw, Burmah.

Schroeder vander Kolk[11] describes briefly a series of rocks collected by Martin in the Moluccas. In the southern part of Amboina the rocks are mainly granite and peridotite, while in the larger northern part they consist of modern volcanics, as they do also on the other islands studied. These rocks are principally dacites and liparites, but on one island andesites occur. Both the dacites and the granite contain cordierite. The dacites are pyroxene and biotite varieties. The andesites are pyroxenic; mica schists, breccias and limestones occur also on the islands. The residue left after treatment of the limestone with acid contains quartz, sanidine, plagioclase, biotite, amphibole, orthorhombic pyroxene, hematite, garnet, cordierite, sillimanite and pleonost.

BOTANY.[1]

The Evolution of a Botanical Journal.[2]—In November of the present year the *Botanical Gazette* reaches its majority, by attaining the age of twenty-one years. It first appeared in November, 1875 under the name of the *Botanical Bulletin*, and consisted of four pages of short notes. It was edited by John M. Coulter, then professor of Natural Sciences in Hanover College (Hanover, Indiana). In his introductory note the editor stated that the object of the new journal was "to afford a convenient and rapid means of communication among botanists. The context shows that it was started as a distinctly western journal, intended to supplement the work of eastern botanical publications.

The first volume included notes by the editor, and Thomas C. Porter, Samuel Lockwood, G. C. Broadhead, M. S. Coulter, Mary E. Pulsifer Ames, J. T. Rothrock, H. C. Beardslee, Coe F. Austin, George Vasey, Alphonso Wood, Isaac Martindale, Elihu Hall, E. A. Rau, and others who have long since diappeared from the botanical field. With the

[9] Neues Jahrb. f. Min., etc., 1866, I, p. 19.

[10] Cf. AMERICAN NATURALIST, June, 1896, p. 478.

[11] Ib., 1896, I, p. 152.

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

[2] Read before the Botanical Seminar of the University of Nebraska. October 10, 1896.

opening of the second volume its name was changed to the *Botanical Gazette*, and the name of M. S. Coulter was added as joint editor. In 1883 by a reorganization of the management, John M. Coulter, Charles R. Barnes and J. C. Arthur became editors, an arrangement which proved to be so satisfactory to the botanists of the country as to become permanent.

The next few years were trying ones for the ambitious editors, but the impetus given to botanical thought by the incoming of modern methods in teaching and study, and perhaps, also, by the organization of the Botanical Club of the American Association for the Advancement of Science, proved helpful in many ways. The Philadelphia (1884) and Ann Arbor (1885) meetings of the Botanical Club created much botanical enthusiasm, the results of which accrued to the benefit of the *Gazette*.

The beginning of its second decade saw it much enlarged, improved in typography and apparently well established in the confidence of American botanists. Year by year it was still further increased in size, better paper was used, and the quality of the matter steadily improved. From the fifty-two pages of short, and mostly local, notes of volume I, we turn to the five hundred and sixty-eight pages of structural, physiological, ecological, systematic and palæontological matter in volume XX. With the opening of the twenty-first volume an additional enlargement was found to be necessary, the numbers averaging sixty-five pages each.

In the earlier volumes there were no plates, the first one occurring in volume VI, illustrating an article by J. C. Arthur on the trichomes of *Echinocystis lobata*. In the twentieth volume there were thirty-seven plates, while for the first half of 1896 the number was twenty-nine.

The last stage in the evolution of this important factor in American botany was reached a few months ago when its financial management was transferred to the University of Chicago. It thus happily becomes an endowed institution, and the editors, relieved from all anxiety as to its business management, are free to develop it along strictly scientific lines. To the three editors whose efforts have given it the foremost place place among botanical journals are hereafter to be added several "associate editors"; at present these are G. F. Atkinson, V. M. Spalding, Roland Thaxter and William Trelease. Under the new regime it promises to be more cosmopolitan than before, and accordingly we are assured that the names of one or more European botanists will soon be added to the corps of editors.

This factor in botanical science has thus been a growth, and it represents to-day much more than so many pages of printed matter. It has grown and developed as the science of botany has grown and developed in this country. When we look over the earlier volumes with surprise at the little notes which fill the pages, we must not forget that American botany had not then generally risen above such contributions. It is true that we had a few masters in the science, with Dr. Gray still in his prime, but these masters wrote little for general reading, and their technically systematic contributions were mostly published in the proceedings of learned societies. The one thing which stands out to-day in sharp contrast with the botany of two decades ago is the very great increase in the number of masters in the science who are making liberal contributions from many different departments. The many-paged *Gazette* of to-day, with its rich variety of matter, differs no more from the four-page *Bulletin* of 1876 than does the botany of the two periods.—CHARLES E. BESSEY.

The North American Species of Physalis and Related Genera.—In a recent number of the Memoirs of the Torrey Botanical Club (Vol. IV, No. 5) Mr. P. A. Rydberg publishes an important contribution to our knowledge of our species of Physalis and related genera. Every one who has attempted to accurately identify any of the native species of Physalis knows well how difficult and discouraging the task has been. Commenting on this Mr. Rydberg says: "The reason is not that the descriptions are so badly drawn, but that only about one half of the actual number of species have, as a rule, been recognized."

After a critical discussion covering fifteen pages the author characterizes the six genera which he includes in his monograph. These are *Margaranthus*, with four southwestern species; *Physalis*, with thirty-nine species; *Quincula*, with one Rocky Mountain species; *Leucophysalis*, with one species of the northern United States and Canada; *Chamæsaracha*, with four species of the southwestern United States; *Oryctes*, with one species from Nevada.

Throughout the paper the nomenclature and synonymy receive full attention, the citations being unusually complete. The descriptions are concise, and apparently drawn with great care. And last, but by no means least, there is a full index of species and synonyms given at the end of the monograph. Altogether it is an unusually good piece of work.—CHARLES E. BESSEY.

Professor Prentiss.—The recent death (August 14th) of Professor Albert Nelson Prentiss of Cornell University calls for more than a mere

brief mention. Born in Cazenovia, N. Y.; May 22, 1836; educated in the Oneida County Seminary, and the Michigan Agricultural College (B. Sc., 1861 and M. Sc., 1864). After short periods of service in the engineering corps of the United States Army, and the public schools of Michigan he became professor of botany in the Michigan Agricultural College (1863 to 1868). After six years of service he was called to the chair of botany in Cornell University (1868), where he remained for twenty-eight years when on account of failing health he was made professor *emeritus* (1896). In these years of work Professor Prentiss was emphatically a *teacher*. The building and equipment of his department, and the training of men who went out to be professors in many colleges, left little time for investigations and the preparation of papers. He chose to impress his thoughts upon men rather than upon paper, and he will be remembered not as a writer, but as a teacher. His life shows how much more effective our work is when we teach men directly by our spoken words rather than through our printed papers.—C. E. B.

The Nomenclature of Mycetozoa.—Professor Mac Bride has been studying the question of nomenclature among these organisms (plants he calls them, and, therefore his results are noticed here) and finds great difficulty in applying the "priority rule" to the solution of the problem. He calls attention to the well-known fact that the earlier botanists did not understand the nature of Mycetozoa and that their descriptions and even their figures in many cases are unintelligible. Rostafinski a little more than twenty years ago gave us the first rational account of the group, and for the first time gave us descriptions by means of which we may know certainly what he had in hand when he applied a particular name. His nomenclature is, therefore, to a large extent the earliest which is authentic. Practically all earlier descriptions are unrecognizable, and therefore, Rostafinski had to take up the work *de novo*. Professor Mac Bride says: "The fact is that when Rostafinski gives credit to his predecessors it is for the most part purely a work of courtesy and grace; there is nothing in the work itself to command such consideration." He therefore concludes that "the man who in his search for priority ascends beyond Rostafinski, does it at the risk of endless confusion and uncertainty in the great majority of cases" and that for these the initial date must be that of his great work, "Sluzowce Monografia" in 1875.—CHARLES E. BESSEY.

The Flora of Wyoming.—Professor Aven Nelson of the University of Wyoming recently issued a valuable "First Report on the Flora of Wyoming," based upon field work in 1892 (by Professor Buf-

fum), 1894 and 1895 by Professor Nelson. The catalogue of plants includes 1118 species of Spermatophytes, 14 Pteriodophytes, 26 Bryophytes, 3 Algæ, 8 Fungi and 7 Lichens, making a total of 1176. The trees of Wyoming are listed as follows: Rocky Mountain Yellow Pine (*Pinus ponderosa scopulorum*), Rocky Mountain White Pine (*P. flexilis*), Lodge-pole Pine (*P. murrayana*), Engelmann's Spruce (*Picea engelmanni*), Blue Spruce (*P. pungens*), Douglas Spruce (*Pseudotsuga douglasii*), Red Cedar (*Juniperus virginiana*), Black Cottonwood (*Populus angustifolia*), Rydberg's Cottonwood (*P. acuminata*), Quaking Aspen (*P. tremuloides*), Sand-bar Willow (*Salix longifolia*), Almond Willow (*S. amygdaloides*), two other species (*S. flavescens*, *S. lasiandra*), Green Ash (*Fraxinus viridis*), Box Elder (*Negundo aceroides*), Scrub Oak (*Quercus undulata*), Wild Plum (*Prunus americana*), Wild Cherry (*P. demissa*), Choke Cherry (*P. virginiana*), Hawthorn, two species (*Crataegus rivularis* and *C. douglasii*), Service Berry (*Amelanchier alnifalia*), Silver Berry (*Elæagnus argentea*), Buffalo Berry (*Shepherdia argentea*), Black Birch (*Betula occidentalis*), Black Alder (*Alnus incana viresceus*), Sage Brush (*Artemisia tridentata*).

The last species is sometimes so large that "a man on horseback may ride erect underneath the branches."

We notice a curious slip by which *Actinella glabra* Nutt. is listed among the new species, although it was published as a new species fifty-five years ago in the Transactions of the American Philosophical Society, and a year or so later appeared under Nuttall's name in Torrey and Gray s Flora of North America, II, p. 382.—Charles E. Bessey.

The Lichens of Chicago.—Bulletin No. 1, of the Geological and Natural History Survey of the Chicago Academy of Sciences is devoted to an enumeration of the lichens of Chicago and vicinity, by Mr. W. W. Catkins. One hundred and twenty-five species are enumerated and very briefly characterized. The paper is supplemented by a useful but incomplete Bibliography of North American Lichenology.—Charles E. Bessey.

Eastwood's Plants of Southeastern Utah.—In the Proceedings of the California Academy of Sciences (2d series, vol. VI) Miss Alice Eastwood enumerates 162 species collected in 1895 in the valley and on the plateaus of the San Juan River in southeastern Utah, a desert region with curious oases about springs and along cañons. Several new species are enumerated, three of which are figured in the plates which accompany the report.—Charles E. Bessey.

Correction.—On page 748, by a slip of the pen the "popple" of the Colorado Mountains is given as *Populus balsamifera candicans*; it should be *P. tremuloides*.—CHARLES E. BESSEY.

Botanical News.—A suggestive pamphlet on "The Pathology of Plants" by B. T. Galloway comes from the Office of Experiment Stations of the United States Department of Agriculture. Its object is to point out certain lines of work in plant pathology that might be undertaken by botanists in the state experiment stations.—From the Division of Agrostology, (U. S. Dept. Agriculture) we have "Fodder and Forage Plants, exclusive of the Grasses" a pamphlet of fifty-eight pages, by Jared G. Smith. It is a descriptive, illustrated list of these plants, written in semi-popular language. It will be of value not only to stock growers, but to scientific botanists as well—Professor W. J. Beal has recently published a Report of the Botanical Department of the Michigan Agricultural College from which we learn that there are in the herbarium 54,243 specimens, and that the botanic garden, begun in 1877 now contains 1335 species.—The Contributions from the U. S. National Herbarium (Vol. III, No. 9) issued August 5, 1896 contains the following papers: The Flora of Southwestern Kansas, a report on a collection of plants made by C. H. Thompson in 1893, by A. S. Hitchcock; *Crepis accidentalis* and its allies, by F. V. Coville; Plants from the Big-Horn Mountains of Wyoming, by J. N. Rose; Leibergia, a new genus of Umbelliferæ from the Columbia River Region, by J. M. Coulter and J. N. Rose; Roseanthus, a new genus of *Cucurbitaceæ* from Acapulco, Mexico, by Alfred Cogniaux.

ZOOLOGY.

Notes on Turbellaria.—1. ON THE OCCURENCE OF BIPALIUM KEWENSE (MOSELEY) IN THE UNITED STATES.

Since the appearance of Moseley's[1] paper in 1878 the species has been recorded from other parts of Great Britain and Ireland, and from Berlin and Frankfurt, A. M. on the continent. It has also been found at the Cape of Good Hope in Africa, in the colonies of Queensland, New South Wales and Victoria in Australia, at Auckland in New Zea-

[1] Moseley, H. N. Description of a New Species of Land-Planarian from the Hothouses at Kew Garden. Ann. Mag. Nat. Hist., Ser. 5. Vol. I, pp. 237–239 1878.

land, Upolu in Samoa and Joinville in Brazil. The wide distribution of this, the largest of land planarians, has doubtless been brought about through the agency of man, the well-marked genus being indigenous only in Japan, China, India, Ceylon, the Malay Archipelago and the East Indies, but this species, *Bipalium kewense*, has never been found in these countries; its home is unknown.

The purpose of this communication is to record the existence of the species in the United States. It is quite abundant in Cambridge, Mass., and has been found there in two different greenhouses. A methodical search would no doubt reveal it in others of the many greenhouses in the vicinity. The largest of the Cambridge specimens measured 300 mm. in length, with a diameter of 4 mm., shorter individuals measuring from 15 mm. upward with the same diameter of 4 mm. The smallest of the specimens always lack the semilunar head end, they being without doubt, the products of reproduction by transverse division in which the head-end had not yet regenerated.

In 1892 Sharp[2] published the description of a Bipalium from a greenhouse in Landsdown, Pennsylvania, which he called *B. manubriatum*. It was suggested by Colin[3] that Sharp's specimen was nothing else than *B. kewense*, for with the exception of the statement that the median stripe is the broadest of the longitudinal markings, the descriptions of *B. manubriatum* agrees in every way with that of *B. kewense*. Variations in the width of the median band in different regions of the same individual of *B. kewense* have been described and figured by Richter[4] and Bergendal,[5] and Dendy[6] has shown the great variability of land planarians within a single species both as regards color and markings. There can be little doubt, therefore, that the single specimen studied by Sharp was the *Bipalium kewense* of Moseley.

The writer would be grateful for any information as to the occurrence of the species in other parts of the United States, and would be glad to have material from other localities.

[2] Sharp, B. On a probable New Species of Bipalium. Proc. Acad. Nat. Sci. Philad., 1891, pp. 120–123, 1892.

[3] Colin, A. Mittheilungen über Würmer. Sitzungsb. Gesell. naturf. Freunde Berlin, Jahrg. 1892, No. 9, pp. 164–166.

[4] Richter, F. *Bipalium kewense* Moseley eine Landplanarie des Palmenhauses zu Frankfurt, A. M. Zool. Garten, Jahrg , XVIII, pp. 231–234, 1887.

[5] Bergendal, D. Studien über Turbellarien. I. Ueber die Vermehrung durch Quertheilung des Bipalium Kewense Moseley. Kongl. Svenska. Vetensk-Akad. Handl., Bd. XXV, No. 4, 42, pp. 1 Pl., 1892.

[6] Dendy, A. Notes on Some New and Little-known Land planarians from Tasmania and South Australia. Proc. Roy. Soc. Victoria, Vol. VI, pp. 178–188, Pl. X, 1893.

2. On the Identity of Procotyla fluviatilis Leidy and Dendrocœlum lacteum Oersted.

Procotyla fluviatilis was first described by Leidy[7] in 1852 under the name of *Dendrocœlum superbum* Girard. In Stimpson's Prodromus[8] (1858) we find for the first time the form under *Procotyla fluviatilis* Leidy M S. with the synonym *Dendrocœlum superbum* Leidy (non Girard). Stimpson's nomenclature evidently being taken from manuscript notes of Leidy, but Leidy himself did not use the name *Procotyla fluviatilis* until 1885.[9] In 1893 Girard[10] in an exhaustive paper on North American Turbellaria makes a new species out of Leidy's first description, which was not his (Girard's) *D. superbum*, calling it Procotyla Leidy (with the synonym *Dendrocœlum superbum* Leidy (non Girard), and also retains *P. fluviatilis* as a second species of Procotyla. In other words, Girard in the same work under two different names gives two different descriptions of the same species. He thus adds greatly to the confusion existing in our knowledge of North American Turbellaria. When our Turbellaria become better known there is reason to believe that the existing large list of species will be much reduced.

A careful study of the structure of *Procotyla fluviatilis* has convinced the writer that this, one of the commonest of our freshwater planarians, is identical with the widely distributed *Dendrocœlum lacteum* Oersted of Europe, and that the genus Procotyla should be abandoned. It was predicted by Hallez[11] that Procotyla would be eliminated when its internal structure should become known. The anatomy and histology of *Dendrocœlum lacteum* has been most carefully worked out by Iijima.[12] His account and figures agree in every way with the American form, as does also the older account of Oscar Schmidt.[13] The variation in

[7] Leidy, J. Corrections and Additions to former Papers on Helminthology. Proc. Acad. Nat. Sci. Philad., Vol. V, pp. 288–289, 1852.

name of *Dendrocœlum superbum* Girard. In Stimpson's[8] Prodromus

[8] Proc. Acad. Nat. Sci. Philad., Vol. IX, pp. 23, 1857.

[9] Leidy, J. Planarians. The Museum, Vol. I, No. 4, p. 5. Philadelphia, 1885.

[10] Girard, Ch. Recherches sur les Planariés et les Némertiens du l'Amérique du Nord. Ann. Sci. Nat., Zool. Tom., XV, pp. 164–166, 1893.

[11] Hallez, P. Catalogue des Turbellariés (Rhabdocœlides, Triclades et Polyclades) du Nord de le France, etc. Revue Biol. du Noru de la France, T. IV No. 11, p. 454, 1892.

[12] Iijima, I. Untersuchungen über den Bau und die Entwickelungeschichte der Süsswasser-Dendrocœlen (Trichleden). Zeitschr. wiss. Zool., Bd. XL, pp. 359-464, Taf. XX-XXIII, 1884.

[13] Schmidt, O. Unterschungen über Turbellarien von Corfu und Cephalonia, Zeitschr. wiss. Zool. Bd. XI, pp. 1–30, Taf. I–IV, 1862.

the number of the eyes in the American form appears to be peculiar, as no mention is made of it in any of the foreign descriptions. In about thirty per cent. of the individuals there are more than the normal number (two) of eyes, the number varying from three to eight, three being the number most frequently occuring.

A detailed account of this and other American Turbellaria, based upon collections made by the Illinois State Natural History Survey and submitted to the writer for study, is in course of preparation.—W. McM. Woodworth.

On the Genus Callisaurus.—Two new species of this genus present lateral fringes of the toes. These are not so well developed as in the species referred to Uma, but they are sufficiently so to show that the latter name must be abandoned, and the species referred to it be placed in Callisaurus. Thus, *Uma notata* Baird, *U. scoparia* Cope, *U. rufopunctata* Cope, etc., must be called *Callisaurus notatus*, etc. The two new species referred to are both from lower California.

Callisaurus crinitus—*Callisaurus dracontoides* Cope, Proceeds. U. S. Natl. Museum, 1889, p. 147. Two series of frontal scales, separated from the rather larger supraoculars by two (or one) rows of small scales. Large supraoculars in four or five longitudinal rows, the inner row largest, the patch bounded by granular scales anteriorly and posteriorly. Interparietal plate longer than wide. Hind leg reaching to front of orbit. Second, third and fourth fingers with well-developed fringes, which are weak on the inner side of the second and third. External side of second, third and fourth toes with well-developed fringes. Femoral pores twenty-three, the scales which they perforate in contact with each other. Color above as in *C. draconoides*. Below a blue patch on each side, with three large oblique black spots and a trace of a fourth. Total length 200 mm., head and body 87 mm., hind leg 72 mm. U. S. N. M., No. 14,895, one specimen.

The differences from *C. draconoides* are the digital fringes, the larger number of femoral pores on adjacent scales, and the three or four black spots of the belly patch; the shorter hind legs, and the longer, interparietal plate. This species has the larger size of the form *C. draconoides ventralis*.

Callisaurus rhodostictus—One row of frontal scales separated by small scales from the rather obscure patch of supraoculars. Interparietal as wide as long. Gular scales subequal. The hind leg extended, reaches to and beyond the end of the muzzle. Well-developed fringes on the external sides of the fingers and toes, excepting on the

first and fifth. Femoral pores fifteen and sixteen, in scales which are separated by intervening scales. Coloration above as in *C. draconoides;* below a blue patch on each side which is crossed by three oblique black spots, the third generally followed by a fourth black spot, which does not reach the abdominal border. In front of the blue patch and posterior to the axilla a large rosy spot. A large rosy spot on the gular region. Size smaller, equal the *C. draconoides draconoides.* Numerous specimens from lower California from A. W. Anthony. As this species was accompanied by *Uta parviscutata* V. den B. and *Crotalus ruber* Cope, the locality is not the Cape San Lucas country. It approaches nearer the *C. draconoides* than does the *C. crinitus.* The differences are, the digital fringes, the three or four black abdominal spots, and the rose spots on the sides and throat.—E. D. Cope.

The Food of Birds.—A report upon the food habits of the catbird (*Galeoscoptes carolinensis*) the brown thrasher (*Harporhyncus rufus*) the mocking bird *Mimus polyglottus*) and the house wren (*Troglodytes aëdon*) by S. D. Judd, contains the following information. The wren is exclusively insectivorus, and, therefore highly beneficial to agriculture. Among the pests destroyed by this bird are the snout beetles, of which the plum curculio is a familiar example. Stink bugs and caterpillars, both of which are plant feeders, are also made way with in large numbers. The catbird and thrasher do much less good than the wren because of their mixed diet of animal and vegetable food, the proportion of the former in the thrasher being 63 per cent., that in the catbird 44, for the entire season. The number of mocking birds examined was only 15, so that their character, as friend or foe of the agriculturist, is still undetermined. The stomachs of those examined, however, indicate that the bulk of their food is animal.

Mr. Judd concludes his report by advising farmers to secure the services of the wren by putting up nesting boxes for them, and protecting them from the quarrelsome English sparrows.

A second interesting paper on the food habits of birds records the results of the examination by Mr. F. E. L. Beal of the stomachs of 238 meadow larks, and 113 Baltimore orioles. The meadow lark is a ground feeder and the great bulk of its food is grasshoppers, of which it consumes an enormous number. The other insects eaten are ants, bugs, caterpillars and beetle larvæ.

The oriole feeds largely on caterpillars and wasps, eating so many of the former that it is a highly important beneficial factor in agricultural work.

A summary of the stomach contents for the whole year shows that nearly three-fourths of the food of the meadow lark for the year, including the winter mouths, consists of insects.

The oriole has a similarly good record. The food for the whole season consisted of 83.4 per cent. of animal matter and 16.6 per cent. of vegetable matter.

These statistics show the importance of according these birds the protection they so well deserve. (Year book Dept. Agri. for 1895. Washington, 1896.

Preliminary Description of a New Vole from Labrador. —In the summer of 1895, Mr. C. H. Goldthwaite made a trip to Hamilton Inlet, Labrador, to collect mammals for the Bangs Collection. The material he got is of much interest, but as I am obliged to delay publishing a full account of it for the present, I take this opportunity of making known apparently the only new species he took—a rather remarkable vole.

MICROTUS ENIXUS sp. nov.

Eighty specimens, all taken in the immediate vicinity of Hamilton Inlet.

Type from Hamilton Inlet, Labrador.

No. 3973, ♀, old adult; collection of E. A. and O. Bangs; collected July 15, 1895, by C. H. Goldthwaite. Total length, 210; tail vertebræ, ·67; hind-foot, 22·5.

General characters: Size medium (about that of *M. pennsylvanicus*); tail long; hind-foot large and strong; colors dark with a sooty brown cast to upper parts; skull differing in many minor particulars from that of any eastern vole; molar teeth extremely small and weak, the tooth row very short; incisor teeth long and projecting.

Color: Upper parts a dark burnt umber brown, with many black-tipped hairs intermixed, and a general sooty cast; nose patch the same. Underparts dark gray (some specimens in fresh pelage slightly washed with buffy). Feet and hands dusky. Tail indistinctly bicolored, black above, dark gray beneath.

Cranial characters: Skull rather small (smaller than the skulls of examples of *M. pennsylvanicus*, the external measurements being substantially) the same; rostrum slender and straight; audital bullæ of moderate size, very round; palate without so pronounced a "step" as that of *pennsylvanicus*. Incisor teeth, both upper and under, long, slender and projecting outward at a decided angle. Molar teeth very weak and small, the tooth row averaging 1 m. shorter than in skulls of

pennsylvanicus of equal size; posterior loop of last upper molar extremely small, enamel folding otherwise much as in *pennsylvanicus*.

Size: Average measurements of ten old adult topotypes—total length 189·4; tail vertebræ, 60·4; hind-foot, 22·4.—OUTRAM BANGS.

Zoological News.—CŒLENTERATA.—Mr. Whiteaves records the finding of a second specimen of the branching Alcyonarian coral, *Primnoa reseda*, in the Pacific waters, off the coast of British Columbia. This is the third species of large Alcyonaria now known to occur in this region, viz., *Verrillia blakei* Stearns, *Paragorgia pacifica* Verrill and *Primnoa reseda* Pallas. Fine examples of each of these are in the Museum of the Geological Survey of Canada. (Trans. Roy. Soc. Canada, Vol. I, 1895–'96.)

PISCES.—A new genus (Apogonops) of fishes from Maronba Bay, New South Wales, is described by Mr. J. D. Ogilby. The genus is founded on a single specimen to which has been given the species name, *anomalus*. At first glance this genus appears to belong with the Apogonidæ, but the absence of vomerine teeth and the number of dorsal spines preclude such a classification. (Proceeds. Linn. Soc. New South Wales, 1896.)

REPTILIA.—Dr. Alfredo Dugés has recently published in La Naturaleza, a useful list of the Batrachia and Reptilia of Mexico, with the localities in which they have been found. While a good many species are omitted, the lists of localities are of much value to the student of geographical distribution.

AVES.—From personal observation M. X. Raspail finds that the time occupied by the Magpie (*Pica caudata*) in the incubation of its eggs is from 17 days to 18 days and 13 hours. The young come from the egg entirely bare, without even a trace of down, and are cared for by the parents about 25 or 26 days before they attempt to leave the nest. (Bull. Soc. Zool. de France, Juillet, 1896.)

The birds collected by Dr. A. Donaldson Smith in Somaliland contain a number of species and genera which find their closest allies in the Cape fauna. In a notice of the collection, Dr. Bowdler-Sharpe states that they are more nearly related to the birds of the Cape than to the fauna of Abyssinia or East Africa. (Geol. Journ. Sept., 1896.)

The collection of birds made by Mr. Abbott in Central Asia has been presented to the National Museum. It numbers 210 specimens, representing 97 known species, and one new to science. The collection has been catalogued by Mr. C. W. Richmond, who embodies in his paper a number of interesting notes on many of the species. (Proceeds. U. S. Natl. Mus., Vol. XVIII, 1896.)

MAMMALIA.—Dr. C. H. Merriam has recently revised the Lemmings of the genus Synaptomys, giving descriptions of three new species. He finds that this genus instead of being monotypic, comprises two well marked subgeneric groups—Synaptomys' proper and Mictomys; that the first of these groups inhabits eastern Canada and northeastern United States from Minnesota to New Brunswick, and contains four fairly well defined forms; that Mictomys has a transcontinental distribution from Labrador to Alaska, and contains at least four species. (Proceeds. Biol. Sc., Washington, Vol. X, 1896.)

ENTOMOLOGY.[1]

A New Era in the Study of Diptera.—The work done on the classification of North American Diptera falls naturally into three periods. The first ended with the publication of the "Catalogue of North American Diptera," by Osten Sacken, in 1859. The descriptive work of most value previous to this time was by Wiedmann and Say, and a little by Loew toward the last. Harris, Macquart and Walker had also published numerous species; but there had been little coöperation, and it was nearly impossible to determine from the descriptions the synonyms that had been created. Osten Sacken recognized this condition, and did not attempt to solve such problems in his catalogue.

The following nineteen years to the second edition of the catalogue in 1878 comprise the second period, characterized by the singular fact that the vast amount of work accomplished was almost wholly by Europeans. Walsh published some twenty species, Riley eight, and several others from one to four each—scarce forty in all—while Loew had in the same time performed the monumental work of describing at least 1300 North American species, Osten Sacken had added several hundred, and Schiner and Thomson a considerable number. Moreover, the new edition of the catalogue was enriched with a vast fund of information gathered by the author in the study of American types in all the principal European collections, revising the synonymy and correcting the generic references as would have been impossible in any other way. About the time of the issuance of the catalogue, the collections of Loew and Osten Sacken were deposited in the Museum of Comparative Zoology, at Cambridge, Mass. This marked the conclusion of what may well be called the Loew–Osten Sacken period. Loew died, and Osten Sacken retiring from the diplomatic service, resumed his residence in Germany. His dipterological writings since

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

1878, while very important, include only one work which describes new North American species—Vol. I of the Diptera in Biologia Centrali-Americana.

In 1879 appeared the first paper of S. W. Williston, inaugurating a new American period, which has continued to the present time. After a few years D. W. Coquillet began to publish, followed by C. H. Tyler Townsend, and he by others, until the number of those who publish occasional papers is now ten or more.

The recent appearance of Dr. Williston's Manual of North American Diptera[2] gives reason to hope that the immediate future will greatly increase the number of workers in this order, so that we will be justified in counting a new era from 1896. It is now possible to determine the genera of nearly all the flies of North America, including the West Indies, with no other work of reference than this volume. More than Cresson's Synopsis does for the Hymenoptera, or Leconte and Harris' volume does for the Coleoptera, this book does for the Diptera, because it includes the territory southward to the Isthmus of Panama. Only the Tachinidæ and Dexiidæ are not tabulated and analytically reduced to genera, and in this confused mass a bibliographical generic list is given, extending to 272 numbers.

While the book purports to be a second edition of the small one published by the same author in 1888, it is practically a new work, having been entirely rewritten, greatly enlarged, and extended to include the entire order with the exception noted. The bibliography since 1878 is given, and all genera not found in Osten Sacken's catalogue have references (in the index) to their descriptions.

The external anatomy of Diptera is very fully treated. Dr. J. B. Smith's interpretation of the mouth parts is given in addition to the usual one, the author not assuming to decide between them. Professor J. H. Comstock's system of wing nomenclature, as used in his manual, is given a place for comparison, but is not used in the work "for two reasons: First, that it has not yet been fully crystallized into permanent shape; second, because nearly all the existing literature has the nomenclature here employed, and to use a new one would largely defeat the object of the work in the hands of the beginner." Baron Osten Sacken's system of bristle-naming or chætotaxy is quite fully set forth. Each family table is preceded by a full exposition of the family characters and a description of the larva, its mode of life, food, etc. (where known).

[2] Manual of North American Diptera. By Samuel W. Williston, M. D., Ph. D. Pp. LIV, 167. James T. Hathaway, New Haven, Conn., 297 Crown St. Paper, $2.00; cloth, $2.25.

The family known heretofore as Blepharoceridæ appears as Liponeuridæ. This change of name was made by Osten Sacken several years ago. He has more recently abandoned the change in a published paper, and there seems no reason why the old name should be displaced.

The families Xylophagidæ and Cœnomyidæ are united with Leptidæ, thus simplifying the family and generic diagnoses. This seems a rather surprising arrangement, yet may be logically defended.

The family Lonchæidæ is united with the Sapromyzidæ. Aside from these changes there are no important differences in the higher categories between the last catalogue and the present work.

While the printing and binding are excellent, there are a number of typographical errors especially in the spelling of generic names, as for instance in Subulomyia, p. 43, the list of lepidopterous genera on p. 146 (five mistakes) and the list of Tachinid and Dexiid genera, p. 147 (four mistakes). But few of these, however, are more than the interchanging or omission of a letter.

This book is Dr. Williston's most important single contribution to dipterology thus far, and it worthily exhibits the industry, experience and ability of the author, which have secured for him world-wide recognition as a dipterist of the highest rank.—J. M. Aldrich, Moscow, Idaho.

Color Variation of a Beetle.—Mr. W. Baterson gives an account of his statistical examination of the color variations of the beetle *Gonioctena variabilis*, which appears to be abundant in hilly places in the south of Spain. He finds that we have here to do with a species whose members exhibit variation in several different respects, and that the variations occur in such a way that the individuals must be conceived as grouped round several special typical forms. There is thus not one normal for the species but several, though all live in the same localities under the same conditions, and though they breed freely all together these various forms are commoner than the intermediates between them. Some time since, when calling attention to the excessive variability of the color of *Coccinella decempunctata* and the no less striking constancy of *C. septempunctata* which lives with it, Mr. Bateson remarked that to ask us to believe that the color of the one is constant, because it matters to the animal, and that the other is variable because it does not matter, is to ask us to abrogate reason. Mr. Wallace, it seems, is of this very opinion; but he does not explain how it is that the color of one is so important, and the color of the other unimportant to the beetle. (Journal Royal Microscopical Society.)

American Nematinæ.—The third of the technical series of bulletins from the U. S. Division of Entomology is entitled "Revision of the Nematinæ of North America, a Subfamily of Leaf-feeding Hymenoptera of the Family Tenthredinidæ." It is by Mr. C. L. Marlatt, and extends over 135 pages, with one excellent plate and several illustrations in the text. We quote from the introduction as follows:

"The subfamily *Nematinæ* of Thompson or *Nematina* of Cameron (Konow's subtribe *Nematides*) comprises a very large group of closely allied species, distributed in the classification adopted by the author among nearly a score of genera. They range from very small insects to medium sized, but include no very large species, or in length from 2 to 12 mm. They are for the most part smooth, shining, and rather soft bodied, and are variously colored, but yet presenting frequently a confusing similarity in general form, and particularly in coloration, rendering their generic and specific references in some cases difficult. In point of number of species and abundance of individuals this subfamily far exceeds any other of the corresponding groups in the family Tenthredinidæ, and in variation and peculiarities in larval habits and in economic importance many of the species belonging to it have a very great interest.

"*Geographical Distribution.*—The Nematinæ are distinctly northern in their range, reaching their greatest development in abundance of species and specimens in the transition and boreal zones, and extend northward into the circumpolar regions—species occurring abundantly in Greenland, Iceland, and Spitzbergen. Southward they become less and less numerous, and are particularly wanting in tropical countries. This is illustrated very forcibly in Europe by the occurrence of over 70 species of the old genus *Nematus* in Scotland (Cameron) and 95 in Sweden (Thompson) as against 12 about Naples, Italy (Costa); and the same discrepancy exists between the temperate and subarctic region of America and the Southern States and Mexico.

"*Food-plants.*—Their food-plants cover a wide range, some species affecting grasses, one or two very destructive to the grains, others various deciduous trees and shrubs, and still others conifers. The majority of the species occur, however, on plants of the families Salicaceæ, Betulaceæ, Rosaceæ, and Coniferæ, in the order given.

"*Life history and habits.*—The Nematines are among the first sawflies to appear in spring, occurring abundantly on trees on the first appearance of the leaves. They do not often frequent flowers, except, at least, those of the plants upon which their larvæ feed. Many willow species, for example, occur abundantly on the earliest spring bloom of the

willow. In common with other sawflies, however, they rarely leave their larval food-plants, and to be collected successfully a knowledge of their habits in this respect is very desirable.

"In number of broods great diversity is found, and the normal rule of most Tenthredinidæ, of a single yearly brood, is frequently deviated from. Some species are known to be limited in number of broods only by the length of the season, as, for example, *Pteronus ventralis* Say, the common willow species. Two annual generations are common, but many species are single brooded, the larvæ entering the soil or other material or remaining in their galls at the completion of growth and continuing in dormant condition until the following spring, when shortly before they emerge as perfect insects the change to the pupal condition takes place. The males normally appear a few days before the females, and the duration of the life of the adults of both sexes is short, not often exceeding a week or ten days. Of a large percentage of the species no males are known, and in the case of many species careful and repeated breeding records indicate that males are very rarely produced.

"In some species parthenogenesis is complete; that is, the eggs from unimpregnated females produce other females. In other instances of parthenogenesis, however, either males only are developed from unfertilized ova or females very rarely.

"The union of the sexes takes place very shortly after the appearance of the females, and egg deposition closely follows. The eggs are inserted either singly or a number together in the young twigs, larger veins, petioles, in the surface parenchyma, or in the edges of the leaves, the single exception being the case of the gooseberry sawfly (*Pteronus ribesii*), which merely glues its eggs to the leaf without making any incision whatever.

"Most of the species are external feeders on the foliage of plants, but the species of two genera, *Euura* and *Pontania*, so far as their habits have been studied, are gall makers, and pass their early life in the interior of the plants, either in the stems without causing abnormal growths or in the excrescences or galls on the stems and leaves. At least one American species develops in the rolled or folded edges of the leaf. The larvæ are 20-footed, some solitary, others gregarious—the latter usually more brightly colored and possessing means of protection in glands secreting a noxious fluid. Most of the solitary ones are green and not readily observed. They usually feed from the underside of the leaves, eating from the edge or cutting circular holes in the general surface, and in some cases taking everything but the stronger veins. Many species rest quietly during the day, feeding only at night.

Some have the habit of throwing the posterior segments violently upward to frighten away parasites or enemies; others adhere to the leaves or twigs by the thoracic feet only, coiling the posterior segments under the middle ones."

Entomological Notes.—Prof. F. L. Harvey monographs in an elaborate manner the Currant Fly, *Epochra canadensis*, in the report of the Maine Experiment Station.

The North American species of Nemobius are monographed by Mr. S. H. Scudder (Journ. N. Y. Ent. Soc., Sept., 1896). Several new species are described.

Mr. Alex. D. MacGillivray has recently monographed the American species of Isotoma in the Canadian Entomologist

In the check-list of the Coccidæ published by Prof. T. D. A. Cockerell, in the Bulletin of the Illinois State Laboratory of Natural History (vol. IV, pp. 318–339) 773 species are listed.

A number of new species of Scarabeidæ are described by Martin L. Linell in the Proceedings U. S. National Museum (vol. XVIII, pp. 721–731.

Prof. J. B. Smith discusses again the San José Scale (*Aspidiotus perniciosus*) in Bulletin 116 of the New Jersey Station.

"The Principal Household Insects of the United Stated" is the title of the extremely valuable and interesting Bulletin No. 4 of the Division of Entomology, U. S. Dept. of Agriculture. It was prepared by Messrs. Howard, Marlatt and Chittenden.

The Lamiinæ of North America are monographed by Messrs. C. W. Leng and John Hamilton, in the Transactions of the American Entomological Society (vol. XXIII, No. 2). In the same issue Mr. William H. Ashmead describes a large number of new parasitic Hymenoptera.

Mr. F. M. Webster discusses the Chench Bug in Bulletin 69 of the Ohio Experiment Station, and several destructive insects in Bulletin 68.

The Phylogeny of the Hymenoptera has recently been discussed by Mr. Ashmead in an interesting and authoritative paper in the Proceedings of the Entomological Society of Washington (vol. III, No. 5).

EMBRYOLOGY.[1]

Movements of Blastomeres.—In a copiously illustrated and extensive paper on the cleavage of *Ascaris megalocephala* Otto zur Strassen[2] lays special emphasis upon certain movements of the cells of the embryo.

In the living egg most remarkable rearrangements of the material are easily seen when the first four cells glide over one another. In later stages changes in form are traced to movements of the cells that must have taken place though not actually seen but inferred from a very detailed study of preserved material. The author confined his attention chiefly to the ectodermal layer of cells and knowing the pedigree of a very large number of them was able to affirm that the changes in shape that the embryo exhibits are due, in part at least, to an actual migration or rearrangement of cells. Cell division and surface tension are not the only factors concerned in this change of position of the cells; there must be some individual movement of certain cells.

This movement of the cells is regarded as being of the same nature as that observed by Roux in the isolated cells of the frog's egg and is, therefore, designated *Cytotropism*.

The production of form in the development of the Ascaris embryo has then this important factor—a power of cells to move towards one another and thus change the shape of the entire mass. This movement is in addition to any purely mechanical movements due to surface tension and is due either to attraction between cells or to repulsion between cells. In either case it is assumed that chemical influences are at work: that this movement arises from *chemotactic* strains.

The movements are much restricted in that a cell travels its own length at most and is never free from its sister-cell. In fact the two cells that arise from the division of one remain connected and are not to be separated by any intrusion of migrating cells and the author thinks that the movements are probably even more restricted in being merely the rearrangements of two groups of such sister-cells both derived from one parent, being merely readjustments of four grand-children of one cell! The entire ectoderm may then be regarded as a mosaic of such sets of families of four, each having its own internal readjustments.

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] Archiv f. Entwickelungsmechanik, 3, 1896, pps. 27–101, 133–188, Pls. V–IX.

Moving pigment in Eggs.—In a carefully illustrated account[3] of the cleavage of the Planarian, *Polychœrus caudatus* Mark, Dr. E. G. Gardiner describes most remarkable changes in position of peculiar, algalike, pigment bodies which color the eggs orange-red for a certain period. These bodies appear in certain cells and then others, they lie along the lines where cleavage is to take place.

They move up from the centre of the egg to the surface and move from place to place.

Fertilization.—By the use of nitric acid Kostanecki and Wierzejski find that the so-called achromatic substance may be demonstrated with remarkable clearness in the eggs of the pond snail *Physa fontinalis.* In a detailed description[4] of radiations, or stars, of this substance seen during the process of maturation of the egg and during fertilization, illustrated by many remarkable figures of reconstructed sections, the authors give the facts that lead them toward the following hypothetical conception of the true nature of the process of fertilization.

The object of fertilization is the union of the nuclei; but the necesary condition to make this of avail is that the egg be able to continue to divide, to undergo cleavage. This power is brought to it by the new nuclear part of the sperm.

Each sexual cell needs to be supplemented by what the other has and it itself is deficient in. This lack is in the protoplasm.

The egg has large amounts of nutritive material while the sperm has none. The former has thus relatively too little protoplasm to continue dividing by itself. During maturation, by dividing twice to form polar bodies, the egg uses up its remaining power of division and must have this added to it again if it is to cleave at all.

What the sperm brings in to replace the exhausted cleaving powers of the egg is the connecting piece of the sperm, the portion near the head or nucleus, that contains achromatic material centered on the centrosome or speck next the head. This material is the remnant from the achromatic figure of the last cell division in the formation of sperms.

This material is conceived of as concentrated and not, as yet, recognized till it gets into the egg; then it swells up and extend in radii as an umbrella unfolds. As the sperm revolves through 180° after entering the egg the middle piece preceeds the sperm head or male nucleus in its journey towards the female nucleus. The middle piece appears as a star centered about the centrosome and rapidly grows in all direction

[3] Journal of Morphology, XI, pp. 155–171.

[4] Archiv. f. Mik. Anat., 47, 2, Apl, 1896, pps. 309--379, Pls. 18--2).

by "assimilating" the net-work of the egg. Thus the star, so remarkably distinct in these snail eggs, about the centrosome of the sperm is to be regarded as at first of male origin and then as gradually getting control of the net-work protoplasm, or the archoplasm, of the egg so that it is eventually the centre of an entire rearrangement of this egg material focussed about the male centrosome.

The centrosome next the famale nucleus disappears and the star about it is "assimilated" by the star that arose about the male nucleus. Sooner or later the male star and centrosome divide to furnish the two-centered system concerned with division of the cleavage nucleus. The male and female nuclei unite to make the cleavage nucleus and the two protoplasmic stars do all that remain to be done in the subsequent cleavage.

The substitution of the new male system for the effete female system of radiate protoplasm is regarded as so complete that the chromosomes in the female nucleus become subjected to the domination of the male system by the growing male radii attaching themselves to these chromo somes by a process of "assimilation" of the old connections, that the author believes to exist between the female chromosomes and the female centrosomes. It is assumed that this male system is all along connected with the chromosomes of the sperm head and that the contractoin of the radii draw the sperm head toward the female nucleus.

Along with the reduction of the chromosomes in both egg and sperm there is probably a reduction in the mass of so-called achromatic substance so that in fertilization there may well be restitution of the normal amount by a mutual supplying of the deficiency.

It will be seen that this conception of the process of fertilization is that of Boveri except that the centrosome is regarded as of no importance and the surrounding, radiated protoplasm becomes the essential factor for cell division. The authors follow Heidenhain in regarding the centrosome as merely the point of insertion of that active, contractile part of the cell that radiates out from this centre.

PSYCHOLOGY.[1]

The Effects of Loss of Sleep.—Prof. Patrick and Dr. Gilbert, of the University of Iowa, have reported in the *Psychological Review* some experiments on this problem. Three normal subjects were kept awake for a period of ninety nours, without resort to stimulants or other

[1] Edited by H. C. Warren, Princeton University, Princeton, N. J.

physiological means. During the four days and three nights of the test they were engaged, as far as possible, in their usual occupations; their meals were of the customary kind, and were taken at the ordinary times, with the addition of a light lunch at about midnight. At intervals of six hours a series of tests was made on each subject, to determine his mental and physical condition. To eliminate the effects of practice, these tests were begun three days before the experiment. The test of the first day of experiment, before any loss of sleep had actually occurred, represent the normal condition of the subject. Tests were also made after the night's sleep that followed the conclusion of the experiment. One of the writers was the first subject. The two other subjects were instructors in the university; the latter were experimented upon at the same time.

Some of the results are of special interest. The reaction time (for sound) showed a gradual increase for two of the subjects, which was masked in the third case by increase of practice; at one period (different in the three cases) the time was considerably greater than earlier or later in the experiment; the mean variation was somewhat above the normal, but not remarkably great. The acuteness of vision, measured by the distance at which a page of print could be distinguished and read, actually increased during the progress of the experiment, and fell off again after the ensuing sleep. The memory test of the two last subjects consisted in committing random series of figures; the time required for this memorizing fluctuated considerably, with a marked lengthening towards the close of the experiment. One of the subjects was unable to memorize the figures at all at two of the last day's tests; he found it impossible to hold the attention upon the task long enough to complete it. The time consumed in adding sets of figures was fairly constant, with two or three exceptions; it was apparently independent of the memory conditions. "Voluntary motor ability," tested by the number of taps that could be made with the finger in five seconds, showed no marked alterations; neither did the susceptibility to fatigue, as tested by continuing this tapping for sixty seconds. The strength of grip, measured on the squeeze dynamometer, fell off from 20 to 30 per cent. at the end of the second day, but afterwards recovered—in two cases fully, in the other partially. The weight of the men remained fairly constant, showing a slight increase towards the close of the period, and the variation of the pulse was within the normal range of daily fluctuations.

The first subject suffered from marked visual hallucinations after the second night. "The subject complained that the floor was covered with

a greasy-looking, molecular layer of rapidly moving or oscillating particles. Often this layer was a foot above the floor and parallel with it, and caused the subject trouble in walking, as he would try to step up on it. Later the air was full of these dancing particles, which developed into swarms of little bodies like gnats, but colored red, purple or black. The subject would climb upon a chair to brush them from about the gas jet, or stealthily try to touch an imaginary fly on the table with his finger. These phenomena did not move with movements of the eye and appeared to be true hallucinations, centrally caused, but due no doubt to the long and unusual strain put upon the eyes. Meanwhile the subject's sharpness of vision was not impaired. At no other time has he had hallucinations of sight, and they entirely disappeared after sleep." Neither of the other subjects experienced these hallucinations.

At the close of the experiment the subjects were allowed to sleep as long as they desired. Tests were made upon the first subject, however, at hourly intervals during the first night, to determine the depth of his sleep. He awoke naturally after ten and a half hours, and remained awake during the rest of the day, but slept two hours more than his normal amount the second night. Of the other subjects, one awoke of his own accord after eleven, the other after fourteen hours' sleep; both felt quite refreshed; they required no extra sleep the next night, and felt no ill effects from the experiment.

It will be noticed that the sleep made up was but a small proportion of the amount lost, viz., 16, 25 and 35 per cent. in the three cases respectively. Two possible explanations for this are offered: either a greater depth of sleep may make up for a lesser duration; or sleep is a relative phenomenon, and the subjects, while apparently awake, were in reality partially asleep at times during the experiment. The authors believe that both of these facts are true, and that they operated together in the present instance. While the subjects were not allowed to go to sleep for an instant, and the slightest tendency to close the eyes was met by active measures, still there were indications of the presence of dreams, in lapses of memory and occasional irrelevant remarks. "It must be understood," say the writers, "that these dreams were instantaneous and the subject as wide awake as he could be kept; but these facts reveal a cerebral condition related to sleep. This hypothesis alone, however, would not seen to account fully for the small proportion of sleep made up. And, indeed, a study of our special tests shows that restoration took place chiefly during the profound sleep following the sleep fast, and took place rapidly. That this sleep was actually more profound, and that the profound part of it was longer than usual, was shown by our experiments in depth of sleep," on one of the subjects.

The authors think it would have been possible to prolong the experiment beyond the ninety hours without danger, except in one of the three cases. These results contrast favorably with those obtained by M. de Manacéine upon young dogs. The animals were kept from sleeping and died at the end of the fourth or fifth day.—H. C. WARREN.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

American Philosophical Society.—November 6, 1896.—The following communications were made: "Recent Archæological Explorations on the Shell Keys and Gulf Coast of Florida," by Frank Hamilton Cushing, followed by Dr. D. G. Brinton and Prof. F. W. Putnam.

November 20, 1896.—Prof. H. V. Hilprecht addressed the Society on his recent archæological discoveries at Nippur, and exhibited a collection of tablets with Summerian inscriptions. A paper on "A New Physical Property of the X-Ray," by Charles L. Leonard, M. D., was read.

University of Pennsylvania, BIOLOGICAL CLUB.—November 2, 1896.—The following demonstrations were made; Descriptive Exhibitive of Streptocarpus and Ephedra by Dr. J. M. McFarlane and of Botrychium by H. C. Porter. The following communication was made; School Museums, by Mrs. L. L. W. Wilson.

H. C. PORTER, *Secretary.*

The Biological Society of Washington.—The following communications were made; Theodore Gill, "The Category of Family or Order in Biology;" C. Hart Merriam, "Notes on the Fauna of Oregon;" E. A. DeSchweinitz, "Some Methods of Generating Formaldehyde, and its Use as a Disinfectant;" C. Hart Merriam, "Supplementary Notes on Tropical American Shrews.

November 21st.—The following communications were made: G. H. Hicks, "The 'Mildews' (Erysipheæ) of Michigan;" Frederick V. Coville, "The In florescence of the Juncaceæ;" Theodor Holm, "The Alpine Flora of Pikes Peak and Grays Peak in Colorado;" C. L. Pollard, "Some Further Remarks on Briton and Brown's Illustrated Flora."

FREDERIC A. LUCAS, *Secretary.*

National Academy of Sciences.—A scientific session of the Academy was held in New York, at the Columbia University, beginning November 17, 1896, at 11 o'clock, A. M.

The following papers were read: "On Certain Positive Negative Laws in their Relation to Organic Chemistry," A. Michael; "The Jurassic Formation on the Atlantic Coast," O. C. Marsh; "The Hydrolysis of Acid Amides," Ira Remsen; "The Isomeric Chlorides of Paranitroorthosulphobenzoic Acid," Ira Remsen; "The Equations of the Forces Acting in the Flotation of Disks and Rings of Metal, with Experiments Showing the Floating of Loaded Disks and Rings of Metal on Water and on Other Liquids," Alfred M. Mayer; "On the Geographical Distribution of Batrachia and Reptilia in the Medicolumbian Region," E. D. Cope; "On the Solar Motion as a Gauge of Stellar Distances," S. Newcome; "Memoir of F. B. Meek," C. A. White; "The Evolution and Pylogeny of Gastropod Mollusca," A. E. Verrill; "On Flicker Photometers," O. N. Rood; "A New Type of Telescope Free from Secondary Color," C. S. Hastings; "A Graphical Method of Logic," C. Pearce; "On Mathematical Infinity," C. Pearce.

A reception was given to the Academy by Mrs. Henry Draper, on the evening of Wednesday, November 18.

Boston Society of Natural History.—November 4th.—The following paper was read: Prof. George Lincoln Goodale, "The Reclaiming of Deserts."

November 18th.—The following papers were read: Prof. George H. Barton, "Observations upon the Inland Ice and the Glaciers Proceeding from it in the Umanak District, Greenland;" Prof. Alfred E. Burton, "The Topographical Features of the Umanak District, Greenland. Other members of the Greenland Expedition were present, and took part in the discussion.—SAMUEL HENSHAW, *Secretary.*

The Academy of Science of St. Louis.—At the meeting of November 2, 1896, Mr. Colton Russell spoke of "What an Entomologist Can Find of Interest About St. Louis," illustrating his remarks by numerous pinned specimens of insects, giving particular attention to the butterflies, and speaking at some length of the phenomena of periodicity, migration, polymorphism, etc., as illustrated by these insects, his paper embodying the result of a large amount of field work performed during the last ten years. Resolutions were adopted opposing the passage of the antivivisection bill now before the United States Senate. Three persons were elected to active membership.

At the meeting on the evening of November 16, 1896, Dr. Charles R. Keyes, the State Geologist of Missouri, read a paper entitled, "How Shall We Subdivide the Carboniferous?" and Professor J. H. Kinealy exhibited a chart for determining the number of square feet of low-pressure steam-heating surface required to keep a room at 70° F., and gave a description of the method of making the chart. Two active members and one life-member of the Academy were elected.

WILLIAM TRELEASE, *Recording Secretary.*

New York Academy of Sciences.—November 9th.—Members of the Columbia University Expedition to Puget Sound made reports on the summer's work.

Mr. N. R. Harrington gave a short narrative of the expedition, including a description of the equipment of the laboratory, dredging, investigation and plankton collection.

In addition, he made a report on the Echinoderms, Crustacea and Annelids. Mention was made of the relation of the asymmetry in *Scutella excentrica* to its habit of burrowing and its vertical position in the sand. Abundant material, both larval and adult, of *Entoconcha* was obtained. This mollusk had been noted by Müller in 1852, and Baur in 1864, in *Synapta digitata* and by Semper in *Holothuria edulis.* The present material was found in an undetermined species of *Holothuria.* About forty species each of Crustacea, Annelids and Echinoderms have been identified.

Mr. Bradney B. Griffin presented the following report on the Platodes, Nemerteans and Mollusks:

The Platodes and Gephyrea are relatively scarce. They are represented solely by two Dendrocoels, and one Phymosoma respectively. The nemertines occur very abundantly, fully fifteen different species were obtained, most of which appear to be undescribed, though some seem to approach more or less closely the European forms rather than those of the east coast of America. The European species are the more numerous.

The Molluscan fauna is very rich and varied, ninety-three species of sixty-nine genera were collected. These include among others the large *Cryptochiton stellerii* which, when alive and expanded measures over 20 cm., besides numerous smaller species of Mopalia, Katherina, Tonicella, etc., that occur in vast numbers on rocks and piles between tides. The Nudibranchs are notable from their bright colors and large size. One species of *Dendronotus* attains a length of over 25 cm. Cases of color variation (*Cardium* and *Acmaea*) and color series (*Littorina*) were to be met with, as well as color harmonization; many

Chitons and Limpets are colored so as to more or less resemble the speckled and barnacled rocks upon which they occur. A complete series of *Pholadidea penita* (the "boring clam") was obtained, which shows the gradual atrophy of the foot and concrescence of the mantle edges as the adult condition is attained. Specimens of *Zirphœa crispata* were collected, a related form in which the foot remains functional throughout life. A series of maturation and fertilization stages of this form was obtained. *Lepton* is not uncommon, a Lamellibranch that lives with commensal attached by its byssus to the abdomen of the Crustacean *Gebia*, and has caused the atrophy of the first pair of abdominal appendages of its host. It has developed a median furrow on each valve in adaptation to the body form of Gebia. An interesting case was observed in which an otherwise nearly smooth *Placuanomia* shell had assumed during its growth the concentric raised lines of a *Saxidomus* valve upon which it was attached.

The insects are not very abundant, they are represented in the collection mainly by a few wood beetles, myriopods (*Julus*, *Polydesmus*), and a species of *Termes*.

Mr. Calkins reported on the Protozoa and Coelenterates of Puget Sound and of the Alaskan Bays.

The Protozoa and Coelenterates collected during the summer by Mr. Calkins belong chiefly to the group Flagellata for the former, and to the Leptomedusæ for the latter. In addition, there are nine species of hydroids—a large number, considering the very limited representation of this group in the western waters. Twelve or fourteen species of Actinians and about the same number of sponges, and several Scyphomedusae complete the list of Coelenterates.

Mr. Bashford Dean reported on the Chordates and Protochordates of the Collection. The Ascidians are represented by about a dozen species, Fishes by upwards of forty. The most important part of his work had been the collecting of embryos and larvæ of Chimaera (*Hydrolagus colliei*) and a fairly complete series of embryos of Bdellostoma, including upwards of 20 stages from cleavage to hatching. Of Chimaera, upward of eighty egg cases had been dredged in a single day; but in every case these were found to be empty. The eggs were finally obtained at Pacific Grove, California, from the female, and were incubated in submerged cages. It was in this locality that the eggs of Bdellostoma were collected.

C. L. Bristol, *Secretary.*

SCIENTIFIC NEWS.

The Hindshaw Natural History Expedition returned to the University of Washington at Seattle, June 15, 1896 from the eastern part of State. The party consisted of Henry H. Hindshaw, Curator of the Museum, Mrs. Hindshaw, and Trevor C. D. Kincaid, Laboratory Assistant in Biology. Transportation was secured for the members of the expedition from the Northern Pacific Railroad, through Surgeon F. H. Coe of Seattle. The party made headquarters at Pasco, where they proceeded to collect a fine lot of plants not found in other parts of the state. In all there were acquired several hundred specimens, covering 150 species. Arrangements were then made for an exploration for Indian relics up the Snake river; and in the meantime Mr. and Mrs. Hindshaw proceeded to the sand hills of Douglas county. These were reached by a trip to Ritzville, thence to Hatton, and from there a drive to the hills of about sixteen miles. Mr. Hindshaw reports some interesting geological facts concerning these sand-hills, or dunes. By an examination of the surrounding blanket or cover of basalt; he concludes that the area covered by these dunes was left uncovered by the general flow of lava making the basalt. This deposit of sand is the layer known as the "John Day" bed. It is the water-bearing rock of Eastern Washington. Farmers come from miles to these sand-hills, where they get plenty of water with little digging. Away from the dunes artesian wells have been sunk. Water is obtained, but sometimes it is necessary to bore through hundreds of basalt to reach the "John Day," or water-bearing, rock. The "John Day" beds carry most interesting fossil bones. Mr. Hindshaw brought home many teeth and bones of the fossil rhinoceros and horse, the latter probably the three-toed ancestor of the present horse. These bones have been worn in the waves of moving sand as badly or worse than is a shell battered by the waves of the ocean. Only the hardest parts of the bones remain, but these are of great interest until further explorations yield more perfect skeletens of these prehistoric mammal remains. After making a thorough search of these sand-hills and procuring all the fossil bones in sight, the party returned to Pasco. Here Mr. D. A. Owen, an enthusiastic collector of Indian curios, had perfected arrangements for a trip up Snake river to Page's ferry and on to the deserted cattle-ranch known as McCoy's. These places were evidently the camping grounds of great bands or villages of Indians before the arrival of the white

man. Many of the old graves are cut out and washed away by the river, but the expedition succeeded in obtaining many valuable specimens, such as stone mortars, pestles, hammers, skin-scrapers, arrow- and spear-points, all of different sizes and shapes. At one place was found a large stone anvil, around which were many fragments of flint and basalt and half-formed arrow-points, showing the remains of a genuine Indian weapon factory. A number of Indian skulls were also obtained. Mingled with these remains and old stone implements, were old brass buttons, blue beads, and an old iron adz, showing that the time of the making of the graves and caches was about the time of the Hudson Bay Company's occupation of the territory in the early part of this century.

Mr. Kincaid secured about 3,000 specimens of insects, comprising about 300 species, which will make a valuable addition to the University's entomological collections.

Thus far the expenses of these collecting expeditions have been borne by individuals, though the University gets the full benefit of the work, and the entire collections.

Other expeditions are planned for the summer months in the various fields of natural science.

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